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IN THE

SCIENCES AND THE ARTS.

EDITORS.

THOMAS ANDERSON, M.D., F.R.S.E.,
REGIUS PROFESSOR OF CHEMISTRY, UNIVERSITY OF GLASGOW;
SIR WILLIAM JARDINE, BART., F.R.S.E.;
JOHN HUTTON BALFOUR, A.M., M.D.,
F.R.SS.L. & E., F.L.S.,
REGIUS KEEPER OF THE ROYAL BOTANIC GARDEN, AND PROFESSOR OF MEDICINE AND BOTANY,
UNIVERSITY OF EDINBURGH.

FOR AMERICA,
HENRY D. ROGERS, LL.D., Hon. F.R.S.E., F.G.S.,
STATE GEOLOGIST, PENNSYLVANIA; PROFESSOR OF NATURAL HISTORY IN THE
UNIVERSITY OF GLASGOW.

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THE

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Notes upon Californian Trees. Part I. By ANDREW MURRAY. With three Plates.*

MY brother, Mr William Murray of San Francisco, having, at various times, in sending home seeds of trees and plants from California, accompanied them with remarks either of his own or of others upon the plants themselves, and sometimes with figures of the more striking, I have thought it might be interesting, and possibly useful to the public, were I occasionally to throw some of these remarks together for publication in this Journal, along with such of the figures as the editors may consider sufficiently interesting to deserve a place in their work. They will necessarily be without order or arrangement, and are not intended for anything more than a record or memorandum of such points of interest regarding the plants as came before him or his friends.

Abies bracteata, Don. Plates I and II.

Having been for some time desirous to obtain seeds and cones of this singularly beautiful pine, I interested my brother in the subject; and although his other avocations prevented his undertaking the expedition himself, he caused two different expeditions to be made to procure it, one in 1856, and the other last season.

The first of these expeditions was conducted by his old fellow-traveller and co-explorer Mr Beardsley; the latter by Mr William Peebles, a gentleman who, in addition to his other qualifications for the task, appears to possess much talent as an artist, for it is to his pencil that we owe the graphic view of this tree as seen on its native mountains.

---

* Read before the Botanical Society of Edinburgh on 13th January 1859.

Before extracting one or two passages from their notes, I should perhaps first remind the reader of what was previously known regarding it.

The tree was first discovered by Dr Coulter, who spent ten years of his life in exploring the wilds of California and Mexico, at a time when they were still undisturbed by the hordes of gold-seekers who have since turned much of the country into vulgar ground. He discovered many new trees and plants, some of which (and among them *A. bracteata*) were described by Mr Don in the "Linnean Transactions," vol. xvii. (1835). Mr Don there says, "This curious and interesting species of fir was discovered by Dr Coulter on the sea side of the mountain range of Santa Lucia, about 1000 feet lower down than *Pinus Coulteri*. The trunk rises to the height of about 120 feet, is very slender, not exceeding two feet in circumference, and as straight as an arrow. The upper third of the tree is clothed with branches, giving it the appearance of an elongated pyramid. The branches are spreading, the lower ones are decumbent." I quote these remarks of Mr Don, because there are some slight discrepancies (nothing material, but still discrepancies) between his account and those given by Hartweg and Beardsley, to be presently adverted to. He goes on,—"The bracts are long and recurved, and but little changed from the ordinary leaves, which give the cones a singular appearance. The seeds are remarkable for a peculiarity in their structure, in having the nucleus exposed at the inner angle of the seed through a considerable opening in the outer testa, as if the junction of the two sides had been prevented by the rapid enlargement of the nucleus." I may remark, that the same peculiarity is to be found in the seeds of *Abies grandis*, *A. nobilis*, and the allied species. Mr Don adds, that it is only the middle branches which bear cones, a circumstance which reminds me of a similar remark made by my brother, on one of his former expeditions, regarding *Pinus tuberculata*, on which he plucked very ancient cones within reach of the ground, growing on the main stem of a great tree. The next botanist who saw this fine tree was Hartweg, who was sent out by the London Horticultural Society in search of plants and seeds in 1847. He describes his various expeditions in reports sent to the London Horticultural Society, which were published from time to time in the Journal of that Society. In vol. iii., p. 225 (1848), he gives the following account of his attempt to obtain seeds of *A. bracteata*. "On September the 20th I again left Monterey for the southern parts, which, on account of the disturbed state of last year, I could not visit before. As guide, I engaged the services of a man who had accompanied me on my last excursion to Santa Cruz, and who, from his profession of a hunter, was well acquainted with the intricate mountain paths of the district I intended to visit. On the day of our starting we reached the Mission of La Solidad, an ill-constructed, half-ruined building, situate in the Salinas Valley, and encamped towards evening on the banks of the Salinas River, within a short distance of the Mission. By sunrise the following morning we were again on horseback, and leaving the main road on the right, we entered a mountain defile leading to the Mission of San Antonio. . . . . From San Antonio a range of mountains extends along the coast, attaining a great elevation, which, though apparently barren, as seen from the Mission, I was assured on the western flank towards the sea is covered by large pines." Mr Hartweg at last reaches the mountain range, where he goes on—"descending the western flank of the great mountain range, I found at last the long-wished for *Abies bracteata* occupying exclusively ravines. This remarkable fir attains the height of 50 feet" (the reader will remember that Dr Coulter stated it at 120 feet, and he will presently find that Beardsley states it at 130 feet), "with a stem from 12 to 15 feet in diameter." This must be a mistake for 12 to 15 inches. Dr Coulter saying that the largest were only 2 feet in circumference, instead of 15 in diameter, which would give 45 in circumference; "one-third of which is clear of branches, and the remainder forming an elongated tapering pyramid, of which the upper part for 3 feet is productive of cones."

These must have been bad specimens, for Mr Beardsley's account describes them as branched to the ground, and Mr Peebles' drawing, although it represents one or two devoid of branches at the base, represents others feathered nearly to the ground. "Having cut down some trees, I found to my regret that the cones were but half grown, and had been frost-bitten. In more sheltered situations towards the sea-shore, the same happened to be the case; and I was thus precluded from introducing this remarkable fir into Europe."

The next attempt to obtain seed of it was that made by Mr Beardsley in 1856—(Jeffrey was not so far south)—and I cannot do better than quote that part of his notes which relates to this tree. He made his attempt in the middle and latter part of October:—"After finishing my collections in this vicinity (Monterey), I set out for the Santa Lucia mountains below the mission of San Antonio; our equipage from Monterey consisted of a waggon drawn by two horses, three loose animals, to ride and pack into the mountains, one Dutchman, one greaser, one rifle, two revolvers, two bowie knives, camping utensils, &c., and provisions for twelve days" (a preparation which indicates that, however much the rest of California is crowded, this part is still in its primitive wildness). "We reached the Mission the third day; here we left our waggon, and proceeded on horseback into the mountains, in search of *Abies bracteata,* which we found on the second day, on the western slope of the range, about 30 miles from the Mission, and about 10 miles from the sea-coast, by the worst trail that I ever travelled in this or any other country. After passing the divide, and descending to the west, I fell in with the tree, occupying the mountain sides as well as the ravines, and not 'exclusively the ravines,' as described by Hartweg; I was greatly disappointed in finding the cones too ripe to be able to obtain a supply of seed; I tried cutting the top off; but a few strokes of the hatchet shattered the cones in pieces, and scattered the seeds to the winds. The only plan was to climb to a most dangerous height and pick off the few cones which could be reached. They went to pieces in my hand the moment they were touched. The cones only occupy a few feet of the top, hence the difficulty and danger of obtaining them. I have never seen any description that does justice to this most beautiful of all the firs; it rises to the height of 130 feet, straight as a line, the trunk tapering regularly from the ground to the top; clothed with branches, which are slim and graceful, down to the ground; the outlines of the branches taper almost as regularly as the trunk, giving the appearance of an 'elongated pyramid,' as Hartweg describes it; but I would rather call it a tall spire with a pyramidal base of two-thirds of the lower part of the tree; the pencil of the artist could not give it a more regular shape than it appears in nature. I saw no tree deprived of its lower branches, except in thickets where it was impossible for them to grow; there was none, with the above exceptions, that I could not step from the ground on to its branches. Not the least remarkable thing is, that these branches bear fine foliage down to the ground, and the branchlets often touch the ground. I have found it occupying exclusively the calcareous districts abounding with ledges of white, veined, and gray marble. We encamped for the night on the point of a ridge, the only place to be found level, and large enough to make down our beds; in the evening, it commenced raining, and increased into a regular driving storm. We passed the most horrible night that ever fell to my lot to experience; we were totally unprovided, as there was no appearance of a storm when we lay down a short time after dark. We had provided wood only to cook with, and we were obliged to get it with great labour, and at the risk of breaking our necks, to keep from freezing. With great difficulty we kept our fire up until morning. The mountains here are as steep as the laws of gravity will admit, and in a state of decomposition; rocks from the ledges above were set at liberty by the rains, and came tumbling down past us, making a fearful crashing among the trees, increasing in speed until they landed among the rocks at the bottom of the ravine below us, with a noise which sent its reverberations up among the hills like peals of thunder. The impenetrable darkness of the night, the howl of the tempest, the crashing of falling rocks, together with the severity of the cold rain, almost snow, made the night truly awful. We saw a large grizzly bear just before dark, and plenty of fresh tracks everywhere, which added nothing to the enjoyments of the night; day-light came at last, and with it a clear sky, which I hailed with more gratitude, I think, than I ever did in my life, thankful that I was alive.

"I had intended to have spent a portion of the day in collecting what few seeds I could; but the storm had beaten them off, so that all attempts in this vicinity were useless. After breakfast, we packed up and took the back track. After passing the first ridge, I descended into a deep gulch where there were a few trees, and found the seeds all gone. I descended again on the north side, and found one small tree that had a few shattered cones left, and obtained about a handful. I attempted to cut off the top, but the first few strokes of the hatchet knocked them all off, and I was obliged to give it up for the season. We reached Monterey after an absence of nine days. We had killed on the trip, four deer, three antelopes, one hare, one wild cat, and seen two grizzly bears."

This account of Mr Beardsley's gives an important hint to cultivators in this country. The tree being confined to a calcareous soil, would indicate that it would be well adapted to our chalk districts, which, for another reason, may be more suitable to it than our Scottish hills, namely, from their better climate. From its locality south of San Francisco, and Dr Coulter's remark that it grows 1000 feet lower on the mountains than Coulteri, it is probably less hardy than the general run of Californian pines. Still Hartweg's finding the cones frost-bitten, and Beardsley nearly getting frost-bitten himself, would seem to indicate that it grows in a climate not free from a considerable degree of cold.

Next year (1857) Mr Lobb, the collector, paid a visit to the spot, but was scarcely more successful than Mr Beardsley. He found that the seeds were, like those of *P. nobilis*, subject to the attacks of an insect in their green state, from which, of course, no precaution in the way of gathering, drying, or packing, can protect them.

Last year Mr Peebles' expedition has produced little more than Mr Lobb's. Notwithstanding that he was a month earlier than Beardsley, he was still too late.

He returned on the 17th of September. He found that the cones were so ripe that the trees could not be cut (which was the usual method adopted in procuring cones from the pines in former expeditions) without scattering all the cones to the winds, so that all he got was obtained by climbing the trees and carefully picking the cones. This difficulty in reaching the spot at the right time is explained by a remark of Hartweg's in regard to other plants. "Being now aware of the rapidity of Californian vegetation, I lost no time in collecting such seeds as were worth taking, and returned to my head-quarters by the beginning of May. Most kinds had during the fortnight after I first saw them in flower ripened their seeds." For the benefit of any future expedition, I may mention that a man of the name of Miers accompanied both Lobb and Peebles as guide, and he knows the station perfectly. It is exceedingly unlikely, however, that any fresh expedition will be tried until the country is more opened up. The expense, danger, chance of bad seed, &c., &c., and small returns, will deter any one from trying it as a mercantile speculation, and there is little in the district to induce an explorer to try such difficult ground which has been already examined.

Mr Peebles mentions that the padres of the Mission use the resin of the tree for incense. My brother sent me a little of it, which I have tried, and find the odour pleasantly terebinthine.

He also sent me a beautiful photograph of the cone taken by Mr G. Johnston, one of the principal photographic artists in San Francisco, which suggests a convenient and rapid mode of procuring information as to species from scientific head-quarters, without the expense and delay of sending bulky specimens, which may possibly be of no value.

The cone does not appear to have been properly figured in any very accessible work. I think, therefore, it will be an acceptable offering to the reader to give the annexed figure, which I owe to the accurate pencil of Dr Greville.

Torreya myristica, Hooker (Californian Nutmeg). Plate III.

This interesting genus was first correctly described and put upon its present basis by Dr Walker Arnott in the "Annals of Natural History" (1st series), Vol. I., 1838. There are two Californian species: one, the *T. taxifolia*, or stinking cedar, is common both to the east and west sides of North America;

the other, the *T. Myristica* or Californian nutmeg, is confined to the western sides of the Rocky Mountains. It receives its vernacular appellation, not from its spicy qualities, nor from possessing any of the intrinsic properties of the nutmeg, but from its cone bearing a very close external resemblance to that fruit, more especially in the ruminated appearance of its seed. Although known to my brother several years before, it was only first sent home by Lobb in 1857, and described by Hooker in the "Botanical Magazine," No. 4780. The fruit and leaves are there figured, but not the tree itself; and as the form and aspect of the tree is of interest on account of its relationship to the yew and pine families, I have thought it desirable to give the annexed figure of it, taken by Mr Peebles. It grows to a height of 30 or 40 feet, and it will be seen that it bears considerable resemblance in habit to an old larch.

On the Action of Hard Waters upon Lead—(concluded).

By W. Lauder Lindsay, M.D., F.L.S.

It will be observed that all the waters examined possessed some action on lead; though only in the cases of the rain waters was this action apparently dangerous in amount or degree.

It has sometimes been ignorantly believed that deposits in lead cisterns, such as I have described, are the precipitated salts of hard waters, which are themselves the means of corroding the metal; and, under this impression, great care has been taken to scrape away the deposit so as to expose a fresh surface of lead. This procedure, of course, has only had the effect of aggravating the mischief! Again, it has been imagined by those who regard such deposit as consisting wholly or partly of carbonate or other insoluble salts of lead, that the supernatant water cannot possibly contain salts of lead, and cannot therefore possess any poisonous properties. Now, even the carbonate, which is undoubtedly insoluble in pure water, may be held in solution, it would appear, in minute quantity—sufficient, however, to possess poisonous properties—if the water is surcharged, as is frequently the case, with carbonic acid. Further, it is not at all necessary that it be held in *solution* in order to the development of its poisonous properties, for it may be simply held in *suspension* in a state of very minute division, in which case there is no doubt of its possessing a poisonous action on the human system. Besides, the hydrated oxide, as well as the chlorides and nitrates, which are frequently, if not always, found in such deposits, are all sparingly soluble in water, and may be, individually or severally, the source of poisonous action. It is sufficiently established, that the chlorides of lime and magnesia are common constituents of spring waters; but it is not perhaps so generally known that the nitrate—according to some chemists, the *nitrite*—of ammonia or other nitrates or nitrites, occur frequently in rain, as well as in river and spring, waters. The mere transparency of a water cannot be regarded as a proof that no lead exists in it. It may appear perfectly pure, and yet contain dissolved hydrated oxide, or other salts of lead, which are slowly thrown down as white and insoluble carbonate on exposure to the air.

A friend in London has furnished me with the particulars of the erosion of a new cistern in his new house at Brompton; and similar cases are common in London. The cistern was roofed with wood; no rain-water entered it; the water supplied to the house was Thames water, from the Chelsea water-works, —a water which he describes to be regarded by laundresses as a soft water, compared with London pump or well water. This water, however, contains nearly 20 grains per gallon of solid matter, 16½ grains consisting of carbonate of lime, and 3 of sulphate of lime and chloride of sodium; while oxide of iron, silica, magnesia, and carbonaceous matter, exist only in minute proportions. There soon appeared, and there continued to be, a copious white sediment on the bottom of the cistern. He made a practice of cleaning out the cistern once a-year; and in about five or six years he found the lead fast becoming eroded over the spots most copiously covered by the white deposit. The remedy he resorted to was plugging the erosions or leakages with gutta percha.

The case related by Dr Wall of Worcester*, and quoted by Dr Christison,† is worthy of record here. A family, consisting of parents and 21 children, were constantly liable to stomach and bowel complaints; 8 children and both parents died in consequence. The house was sold after their death, and the purchaser found it necessary to repair the pump, the cylinder and cistern of which were riddled with holes, and as thin as a sieve. The plumber employed informed Dr Wall that he had repaired it several times previously. Unfortunately, no analysis was made of the water, and we are left to infer that it was *hard*, from the fact that most of the water about Worcester is hard. This, however, does the reverse of account for its action on lead: hence the cause in this case was supposed to be one of the three following:—1. An unusually small proportion of salts; 2. An excess of carbonic acid; or, 3, "Which is perhaps fully more probable, the corrosion was the effect of *some obscure galvanic action!*" Now it is doubtless very convenient to ascribe or attribute cases, otherwise inexplicable, to "obscure galvanic action;" but it is just to the rationale of this, and similar *obscure* actions of water on lead, that I am desirous of directing the attention of chemists. Many cases similar to that given by Dr Wall are on record; but they are comparatively valueless, from the want of an analysis of the waters concerned. It is extremely desirable that, in all future records of cases, there should be given some key to the character of the water. If nothing further can be stated or ascertained regarding it, the narrator would do service by mentioning whether it is hard or soft—whether it curdles soap, or forms a lather therewith. Dr Christison says,—"Of spring waters, which act with inconvenient or dangerous rapidity on lead, *several instances* might be quoted. But it is hardly worth while mentioning more than one or two by way of illustration: because the nature of the water has been very seldom described so carefully as to supply the means of explaining their operation."*

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* Transactions of London College of Physicians, ii. 400.

† Treatise on Poisons, 1835, p. 488.

It may be asked, in the case of cisterns so eroded, and which must continue to contain the same kind of water, How is the tendency to erosion to be obviated? In the cisterns mentioned at p. 252, it was obviated, after repeated solderings and patchings, by coating the lead with a composition of rosin and tallow, which has answered the desired purpose admirably. This is the common mode of rectifying the mishap in this quarter (Perth). Plumbers, then, use a very suitable *mechanical* means of protecting the lead; but a very ingenious *chemical* means has also been recommended by several distinguished authorities. It has been found that different neutral salts protect lead from the corrosive action of water with very different degrees of efficiency—chloride of sodium in the proportion of $\frac{1}{2000}$th, and sulphate of lime in that of $\frac{1}{4000}$th. Of these, one of the most powerful is phosphate of soda, a very small quantity of which added to a water, prevents its action on lead. Hence it has been recommended to add to a water which is, or is supposed to be, corrosive of lead, a given proportion of this salt (about $\frac{1}{25000}$th part). Professor Christison has pointed out that this so-called "preservative power" depends on, or resides in, the *acid*, and not the base, of the salt. For instance, the *sulphates* of soda, magnesia, and lime, as well as the triple sulphate of alumina and potash, preserve, as nearly as can be determined, in the same proportion. In regard to their relative preserving power, it is found that salts, whose acids form with lead a soluble salt, are least energetic, and *vice versa*.* Other mechanical means might be suggested to remedy the evil complained of,—such as the substitution of cisterns of other metals, or of wood, earthenware, glass, or gutta-percha. Or the lead may be lined with gutta-percha, india-rubber, or other materials. But my object at present is merely to indicate fully the evil, not to suggest a remedy.† For an illustration of the deleterious effects on the human system of hard water contaminated with lead, I am indebted to the kindness

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* Treatise on Poisons, 1835, p. 488.

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* Treatise on Poisons, 1835.

† From communications received in relation to the above paper, as it was laid before the late meeting of the British Association, I find that my ideas regarding the coating of the interior of lead pipes and cisterns with substances on which water exerts no action, or, at least, no deleterious action, have been anticipated by the patents of Mr E. K. Davis, of 42 Tenison Street, York Road, Lambeth, London. The lead he manufactures and lines he guarantees to resist the action of waters of all sorts, as well as of beer and similar fluids; and his patents extend to England, France, and Austria. The essential principle of his patents is simply the lining or coating of the lead with substances not acted upon by water, beer, and other fluids, and which cannot, therefore, confer upon such fluids poisonous or deleterious properties. The materials he uses are various, specimens of which I have had an opportunity of examining for myself through the kindness of the patentee. I have accordingly every confidence in recom-

mending Mr Davis's patents to the notice of all who are charged with, or concerned in, the conveyance or storing up of water, beer, &c., for culinary or drinking purposes. The materials Mr Davis uses are chiefly of two classes, viz.:—

1. Pure tin. A special machinery is applied; the lead pipe or sheet is operated on in a solid state, but at a heat above the melting-point of tin. The fused tin and lead are firmly united, it being easy to make the tin coating of any desired or desirable thickness. If the coating be comparatively thick, then there is virtually a double cylinder or tube,—the outer or thicker being of lead, the thinner and inner of tin. It is perhaps necessary to explain here that such pipes are quite different from what are known to plumbers as "tin-coated pipes." In the latter case, the coating is very thin, and consists of what is termed "fusible metal,"—a mixture of lead, bismuth, and tin.

2. Compositions of various gums, resins, gutta-percha, caoutchouc, bitumen, and similar materials, selecting such only as form an adhesive and flexible film, which will not be ruptured or abraded by the processes of bending and otherwise manipulating the lead. The specimens of pipes I have examined are first coated internally with a composition of several of the materials just named, and subsequently lined by a film of gutta-percha. Gutta-percha or caoutchouc cannot be directly applied as coatings, from their non-adhesiveness to metal when precipitated from their solutions. But they adhere firmly to a surface of any of the compositions prepared by Mr Davis.

In reference to Mr Davis's patents, the *Builder* remarks:—"The most hopeful and efficient remedy, however, now practically available, appears to be that provided under a patent secured by Mr E. K. Davis of Lambeth, for the coating (amongst other processes) of the interior of leaden water-pipes with a thin film or inner tube of tin, or of compositions of gum-lacs and india-rubber or gutta-percha, in a soft or fluid state, by means of improvements in the usual machinery for making leaden pipes themselves. The coating appears (in pieces of the various tubes which we have purposely examined) to be perfectly adherent, of quite sufficient thickness, and otherwise efficient in its formation for the purpose in view."

"The mechanism by means of which these and other processes are effected is ingenious; and the patent of Mr Davis comprehends various improvements, —such as a double-action hydraulic press, the casing of soft metal pipes with block-tin and other metals, the forcing of soft metals by compressed air, the plating of sheets in a way described, the making of dies for coating lead and other pipes with other metals, a mode of making still-worms, the coating of soft metal pipes with gutta-percha, india-rubber, &c., and various other improvements. It is with special reference to a remedy for the contamination of water by lead that we have examined the pipes coated under this patent; and we think it well deserves the public patronage, as tending towards the realization of that very desirable end."

The London plumbers, who are familiar with the fact that new cisterns in London houses are frequently eroded to the extent of leakage in the course of one or two years, usually apply as a remedy a coating composed of rosin, of a lady in Derbyshire, in whose household the following incident occurred.*

"Mr B——, the gentleman, who acted as tutor to our children last year, came to us soon after Christmas. He had previously been in perfect health, so far as regarded the digestive organs, although the localities in which he had been residing—Edinburgh during the College session, and Glasgow as his home—did not offer any peculiar advantages in this respect. Soon after his arrival he complained of dyspepsia in no ordinary degree. The usual remedies had no effect. A strict system of diet proved equally inefficacious. After exhausting the skill of our country practitioners, he went to Manchester to seek for relief there. Mr ——, a surgeon of large practice there, after a careful investigation of his mode of life, his symptoms, &c., told him that he could not account for the attack except on the supposition of lead being held in solution by the water. Mr B—— objected that he was the only invalid in a family of nineteen, and that he believed the water was from a spring, and conveyed to the house by iron pipes. Mr S—— rejoined that the conditions of his ordinary life were so favourable to health, the hours of the meals so appropriate, and the remedies which had been used so fitted to benefit a case of pure indigestion, had there been no irritating cause, continued that the presence of lead in the water offered the only solution of the difficulty. On his return, we drew four bottles of water from four different taps within the house, and sent them to Professor —— for investigation.

"The origin of the water with which we are supplied is rather singular. Far up on a high hill two springs, within ten or twelve feet of each other, rise, and mingle their water a few yards lower down. One of them is *excessively hard;* the other is *equally soft.* Both flow together into a reservoir; thence filtering through gravel, they pass into *iron* pipes,

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white lead, and grease! This, it need hardly be observed, is a most objectionable remedy compared with the substances enumerated in Mr Davis's patents.

Dissolved india-rubber has, however, been applied to a similar purpose; and it has been suggested to me by an ingenious correspondent that a varnish of coal-tar, which dries hard and fast, and is deemed a most serviceable coating for exposed iron and other metallic work, might also be used.

* Narrative contained in letter dated 14th July 1858.

which, descending into a deep valley, and crossing it, rise again to the hill on which our house is situated. It is carried up to the very top of the building, and is there deposited in a *lead* cistern, from which some leaden pipes lead it down to the butler's pantry and the place from which we used to get our drinking water. The kitchens, &c., were supplied from the iron pipes before the water ascended to the top of the house. The house is very large, and the roof is flat. The cistern occupies a large space on the top, and is of course exposed in its full extent to the air.

"Professor —— detected lead in the two taps *leading from the cistern;* the other two he declared to be perfectly free from it. The bottles were simply labelled with initial letters. We were very incredulous, and, on expressing our disbelief, he asked us to send the water again differently labelled. The result was the same as before. We immediately removed the filter to one of the kitchen pipes [the water from] which had been declared free from lead. In a short time Mr B. was decidedly better: he looked very much better, his colour clearer, and his whole appearance improved; but he said he still felt occasionally some touches of his old complaint, although nothing in comparison to what he formerly endured. He has gone to Glasgow for the holidays, and wrote to me the other day that 'he ignores the very name of indigestion.'"

In a subsequent letter,* explanatory of some details, the same lady informs me:—"We consider the water *hard,* although, as I mentioned before, there is a *soft* spring mingled with the *hard* one. . . . . Lead was found in this *hard* water *after* it had been conveyed into a leaden cistern and distributed thence in leaden pipes through part of the house. *No trace of lead* was found in the same water, which was also in the house, but flowed through *iron* pipes only."

According to the analysis of Professor ——, the water drawn before it entered leaden pipes or cisterns contained of

Organic matter, per gallon, . .	3·50 grs.
Inorganic, " . . .	11·37
	14·87

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* Of date 2d August 1858.

The latter consisted of the following salts:—

1. Potash.—*a*. Chloride—very small quantity.
    b. Sulphate—a trace.
2. Lime.—*a*. Sulphate—rather abundant.
    b. Carbonate—small quantity.
3. Magnesia.—*a*. Chloride—rather abundant.
    b. Sulphate—small quantity.
    c. Carbonate—small quantity.
4. Iron.—*a*. Carbonate—very small quantity.
5. Ammonia.—*a*. Nitrate—a trace.*

The analyst designates the water "very soft," and reports it as fit for all domestic purposes, in the absence of lead; while Mrs B., in her letters above quoted, repeatedly speaks of it as *hard!* It is now well known that the quality of hardness in water does not depend merely on the *amount* of its saline ingredients, but chiefly on their *kind* or *nature*. Professor Graham points out that some London waters are *soft*, though containing as much as $\frac{1}{1240}$th of solid matter.†

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* Date of analysis, October 10, 1857.

† At my request, two beer bottles full of the waters in question,—the one specimen (No. 1) drawn from lead pipes issuing from a lead cistern containing the water, and the other (No. 2) taken from iron pipes, the water not having in this case been at all contained in, and not having passed through, leaden vessels, were kindly sent to me for analysis in the beginning of August last; but I was unable to overtake their examination till 20th September. My results were as follows:—No. 1 had, on drawing the cork, a distinct smell of sulphuretted hydrogen; and the water contained a quantity of brown flocculent matter, apparently derived from the cork. A stream of sulphuretted hydrogen was passed through the water, both in its original state, as received, and after it had been greatly concentrated by evaporation. Not a trace of lead was discovered by this or any other process applied; neither was iron detected by the rough qualitative methods employed. There was a small proportion of sediment on evaporation. Carbonates were comparatively abundant; chlorides and sulphates in very small quantity; lime and magnesia were plentiful, the former especially.—No. 2 also had a slight smell of sulphuretted hydrogen. The water gave precisely the same reactions as No. 1, except that the sulphates and carbonates were rather more abundant.

It will be observed that there is some discrepancy between the analysis originally made by an English chemist and my own results as just given. This is not to be wondered at, when we consider the changes in the composition of waters produced by season, rainfall, drought, &c. A fall of rain, for instance, must greatly modify the composition of waters, especially if they are derived from the surface drainage of an agricultural district. The tables furnished by Dr R. D. Thomson of St Thomas' Hospital, London, and published in the Reports of the Registrar-General of Births, Deaths, &c., show distinctly

I was recently informed by a friend in London, who formerly lived in Brompton, that he constantly or repeatedly suffered from anomalous stomach complaints while he resided there, and which he attributed solely to the water with which his house was supplied being contaminated with lead; of this, however, he had no chemical proof. The water he described as hard: it was supplied by one of the London water companies,—the water furnished by all of which contains, in comparison with the waters of Edinburgh or Aberdeen, large quantities of solid or saline constituents. He only got rid of his symptoms by removal to Clapham, where the water supply is derived from a garden-well, sunk in the gravel of the London basin. This water also is described as hard; but it is not contained in leaden cisterns, nor does it pass through leaden pipes. The same gentleman, who is connected with the Metropolitan Board of Works in London, and who has an ample experience of such matters, informs me that such cases are very common in London,—so common, indeed, that he laughed at the idea of hard waters protecting lead.

I will now briefly review the various theories or explanations that have been, or that may be, advanced to account for the action of spring waters, which are generally more or less hard, upon lead. They may be tabulated as follows:—

1. Unusually small quantity of
   - *a.* The neutral salts generally, or in the aggregate.
   - *b.* Particular neutral salts, such as the carbonates and sulphates.
2. Unusually large proportion of carbonic acid in the water.
3. The presence of acids derivable from decaying vegetable or animal matter.
4. Presence of foreign bodies or accidental impurities.
5. Presence of nitrite of ammonia.
6. Presence of atmospheric air in the water, perhaps in unusual amount.
7. Use of leaden covers to cisterns.

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the difference in the composition, from month to month, of the same waters, supplied by the same metropolitan water companies, and derived from the same sources. Dr Medlock remarks,—"It not unfrequently happens that a water which, at one season of the year, acts vigorously upon lead, has no action upon it at other seasons." (*Ol. citat. p.* 33.)

8. Long exposure of still water to the same surface of metal.
9. Great extent of surface exposed.
10. Galvanic action from the contact of different metals, or of different qualities of the same metal, under water.
11. Chemical or electro-chemical causes, with which we are at present unacquainted.

With a knowledge of the statements of Professor Christison as to the power of the neutral salts generally to protect lead from the corrosive action of water, it is unnecessary to illustrate the first half of the first proposition or cause above tabulated, because a water deficient in such salts approaches the characters of rain or distilled water. And further, with a knowledge of what has been determined in regard to the different protective powers of particular neutral salts, it is easy to comprehend how a water containing abundance of chlorides, but a minimum of sulphates and carbonates, should act upon lead nearly as readily and rapidly as if there were an absolute deficiency of all neutral salts.

Professor Daniell says that *all* waters will corrode lead if they contain *excess of carbonic acid.* " My friend, Professor Daniell," says Dr Pareira, " informs me that he has found lead in the well-water obtained at Norwood. The water is *very hard* (that is, holds a large quantity of sulphate of lime in solution), and contains much free carbonic acid. It is the latter ingredient apparently which holds the lead in solution; for, by boiling, the whole of the lead is precipitated. The water is raised from the well by a leaden pump, to which is (?) attached a few feet of leaden pipe. Professor Daniell's attention was directed to the subject in consequence of the occurrence of several cases of lead colic in the neighbourhood of his residence at Norwood."* Professor Taylor opposes the statement, that carbonate of lead is at all soluble in water charged with carbonic acid. " It has been supposed," says he, " that carbonic acid contained in water would partially dissolve and suspend the carbonate in it; but in saturating water with carbonic acid, over finely-divided carbonate of lead, it was not found, on filtration, that any perceptible portion had been dissolved."* "Natural water," says Professor Fownes, "highly charged with carbonic acid, *cannot under any circumstances*, be kept in lead or passed through lead pipes with safety; the carbonate, though very insoluble in pure water, being slightly soluble in water containing carbonic acid."†

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* A Treatise on Food and Diet. By Jonathan Pereira, M.D., &c. P. 96. London, 1843. See also Morson in Pharmaceutical Journal, November 1, 1842.

It has been supposed, and with every prospect of the supposition being correct, that the presence of decaying leaves and similar organic matters, whether of vegetable or animal origin, may favour the corrosion of lead by the development of various *acids* due to the gradual decomposition of such organic matters. Pareira mentions a case in which a fragment of mortar appears to have aided or determined the corrosive action of the water.

"The presence of *nitrite* of ammonia in water," says Dr Medlock, "I have clearly established by the most conclusive experiments; and it is to the presence of this substance, both in distilled waters and waters selected for domestic use, that the action of such waters on lead is due. The nitrous acid attacks the lead and forms the soluble *nitrite* of lead, which, by exposure to air, combines with carbonic acid, and precipitates the oxide of lead in the form of insoluble carbonate of lead, setting free the nitrous acid again to attack and dissolve a fresh portion of lead."‡

It has been suggested that the presence of an unusual amount of atmospheric air *in water* would favour or facilitate its action on lead. Doubtless it would do so were it proved to exist, but this cannot be an important element or agent in the action in many cases. The presence of air *externally to* the water is as much an essential to the action as is that of the water itself; for, unless there is free exposure to the air, oxidation and the subsequent changes in the metal will not proceed. Water is well known to be capable of holding in solution a considerable proportion of air and other gases. Rain-water, remarks Professor Christison, generally contains 2½ per cent. of its bulk of air, in which the proportion of oxygen gas is so high as 32 per cent. In water from freshly-melted snow the proportion of oxygen is 34·8 per cent., according to the observations of Gay-Lussac and Humboldt, while the oxygen in atmospheric air does not exceed 21 per cent.* Professor Christison has also pointed out that distilled water deprived of its gases by ebullition, and excluded from contact with the air, has no action on lead.†

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* On Poisons. London, 1848. P. 450. "Water Poisoned by Lead."

† Manual of Elementary Chemistry. London, 1856. P. 333.

‡ On the Action of Certain Waters upon Lead. Record of Pharmacy and Therapeutics, Part II. London, 1857. P. 34.

The use of leaden covers to cisterns is fraught with great danger, and fortunately is comparatively seldom resorted to by plumbers. If cisterns are covered at all, probably the safest material is wood. However impure or hard a water is, however saturated with sulphates and carbonates, and however free from plumbeous impregnation, the moisture which results from its evaporation, and which condenses on the leaden cover, is pure or distilled water. Hydrated oxide and carbonate are produced in the ordinary way; these gradually encrust the lead, and by-and-by scales or fragments fall off and drop into the subnatant water, in which they are partly dissolved, partly held in a state of suspension.

Time is a most important element or condition in the action of water on lead. The mere flowing of water through leaden pipes, and the standing of water for a long period in leaden cisterns, are two very opposite conditions. Accordingly, it is found that the same water which will cause no perceptible change on lead from short contact, will produce a copious deposit of oxide and carbonate on standing for some weeks or months. Professor Christison remarks this in regard to Edinburgh water, which contracts no material impregnation after a few days' contact. A cistern in his laboratory in the University of Edinburgh was left undisturbed for some five or six months with a few inches of water in it. After the lapse of this period, he found "so large a quantity of pearly crystals lying loose on the cistern, and diffused through the water, that, when the whole was shaken up and transferred to a glass vessel, the water appeared quite opaque."‡ I believe that such is the action of Edinburgh water,—a water containing about 12 grains per gallon of solid matter,* that in the course of ten days or a fortnight, while at rest in contact with lead, it will dissolve the metal to the extent of about $\frac{1}{500}$th of a grain at least per gallon. Professor Christison again remarks,—"Neither ought ordinary terrestrial waters to be *kept very long in leaden vessels;* because the same changes, though not appreciable in a few days or weeks, are nevertheless accomplished in the course of time."† The late Dr Thomson of Glasgow says, when he lived in Edinburgh, he "could always detect a minute trace of lead suspended in the water, which at that time was brought six miles in leaden pipes."‡

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* Trans. Roy. Soc. Edin., Vol. 15, Part II., p. 271.

† Treatise on Poisons, 1835, p. 476.

‡ Treatise on Poisons, 3d edit. 1835, p. 491.

An equally important element in the action is perhaps the *extent of surface exposed.* It not unfrequently happens that experiments on the small scale give erroneous, and consequently dangerous, results, merely because the surface exposed was too small and the time allowed too short. Professor Christison gives an apt illustration. A gentleman in Dumfriesshire had resolved to introduce into his house, through leaden pipes, the water of a spring some three-quarters of a mile distant. But, with a view to ascertain whether it was quite safe to do so, the usual experiment was made, of placing a few freshly-scraped or polished leaden rods in contact with the water for a few days, At the end of a fortnight, the result was imperceptible; from which it was forthwith concluded that this water might be conveyed through lead pipes and stand in lead cisterns with all safety.§ The works were immediately carried out; but the results were as dangerous as they were unexpected. The water was soon found to be impregnated with lead to such a degree as to be opalescent. Analysis showed the water to have been extremely pure, containing not more than $\frac{1}{22000}$th of saline matters, which consisted mainly of sulphates, chlorides, and carbonates. Here the unintentional experiment with 784 square feet of lead produced very opposite results from that with a few inches! *

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* As a comparative statement, I may here mention that, in round numbers, the water of Glasgow,—supplied from Loch Katrine,—contains from 1 to 2 grains per gallon of solid matter; that of Aberdeen 2 to 4; and that of London from 15 to 30!

† Dispensatory, 2d edit. 1848. Art. *Plumbum.*

‡ Christison's Treatise on Poisons, 1835, p. 487.

§ "Water, which tarnishes polished lead when left at rest upon it in a glass vessel for a few hours cannot be safely transmitted through lead pipes without certain precautions. Conversely—*it is probable, though not yet proved,* that if polished lead remain untarnished, or nearly so, for twenty-four hours in a glass of water, the water may be safely conducted through lead pipes."* Taylor,

* Christison, Trans. Roy. Soc. Edinb., vol. xviii., part ii., p. 271.

Galvanic action in connection with the corrosion of lead seems to me to be a most hopeful field of research. It is generally called into requisition to explain all anomalous cases of the action of hard waters on lead. But this is making too much of it. I have no doubt that it exerts a powerful influence in many cases, but it has yet to be distinctly shown in what cases, how and under what circumstances, it operates. The subject is by no means new. Dr Paris long ago drew attention to it, and it has also been taken up by Professors Christison and Pareira; but it is still, to say the least of it, very imperfectly understood. I would therefore strongly recommend it to the study of chemists and electricians. The contact of other metals with lead, the use of solder, the mere existence of impurities or inequalities in the lead, seem to determine galvanic action; in which event the lead "becomes more susceptible of, and exposed to, the agency of electro-positive elements, among which are alkalis and alkaline earths, and these exert considerable solvent power over it."† "Galvanism," says Professor Christison, "is a most important co-operating agent, or rather, perhaps, it should be considered a distinct power, for it acts with energy where water alone acts least,—viz., where there is saline matter in solution, because then a gal-

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too, regards the freshly-scraped piece of lead as a good test of the absolute purity of distilled water; and, conversely, of the liability of water to poisonous impregnation with lead. If the fresh metallic surface, says he, remain bright after some days, or only acquire a faint incrustation of sulphate, and if sulphuretted hydrogen does not give a brown tint to the water, there is but little danger of its becoming poisoned with lead.*

* Treatise on Poisons, 3d edit. 1835, p. 489.

† Pareira on Food and Diet, p. 97, and Brande's Dict. of Mat. Med. and Pract. Pharmacy, p. 80. London, 1839.

* On Poisons, London, 1848, p. 450—"Water poisoned by lead."

vanic current of greater force is excited. . . . Even inequalities in the composition of the lead may have the same effect. Sheet-lead, long exposed to air or water, is sometimes corroded in particular spots; and these will always be found in the neighbourhood of parts of the metal differing in colour, hardness, or texture, from the general mass. This, however, is a *mere supposition;* but it affords ready explanation of the corrosion. Similar effects may arise simply from fragments of other metals lying long in contact with lead. . . . *I have no doubt* that many of the instances of unusually rapid corrosion of lead by water,—such as that mentioned by Dr Wall, are really owing not to the simple action of water, but to galvanic action, excited obscurely in one or other of the ways now mentioned."*

In the course of a lecture on "Public Health," delivered in the City Hall, Perth, on 4th January last, Dr Stirling of Perth made the following statement in regard to the influence of galvanic agency in aiding or determining the corrosive action of various waters, containing saline matters, on lead:— "Having compared the effect produced on lead rods immersed in solutions of various neutral salts, in distilled water, with that produced on similar rods in contact with copper wire, immersed in solutions of the same salts, and of the same strength, I am led to the following conclusions, viz.—1. *That galvanic action is a most powerful agent in promoting the corrosive action of certain waters upon lead;* 2. That, in water containing a small proportion of certain feebly protective salts, galvanic action is slightly, if at all, produced; 3. That, in water containing the same salts in so large quantity as to be considered protective to the metal, galvanic action is so powerfully induced as to render the water, instead of innocuous, nearly as corrosive as the very weak solution; 4. That, in water containing certain powerfully protective salts, galvanic action is sufficiently produced to render it doubtful whether any saline solution can altogether protect lead from corrosive action, when, from the nature of the containing vessel, or other cause, galvanic currents may be induced."

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* Treatise on Poisons, pp. 494–5.

In the discussion following the reading of this paper at the British Association, several interesting illustrations were given of corrosive action on lead and copper, attributable, or supposed to be attributable, to galvanic action. For instance (I quote from a Leeds newspaper), "a member said that when copper sheets from different factories were joined together to cover the bottoms of ships, strong galvanic action was set up when they were immersed in sea-water. When all the sheets on the bottom of the ship were of the same kind of copper, the corrosion was very slight. As lead was affected in a similar manner, the sheets of that metal used for lining cisterns *ought always to be of one kind.*"

"Dr Edwards attributed the different action of the same water on different lead cisterns to the fact that some sheets of that metal were made from old lead, containing an admixture of solder, which caused it to be corroded by galvanic action. It was shown that thirty or forty years ago, cisterns were not so much acted on by water as now; and it was probable that this was owing to the fact, that the silver in lead was now extracted, whereas formerly it was not worth while to do so. The long corroded streaks which were often observed in sheet-lead were caused by galvanic action set up by small portions of tin which had been attached to old lead."

"The Rev. W. V. Harcourt could not understand how the presence of silver in lead could affect it much, considering the electrical relations the two metals bore to each other. In his opinion, it was *advisable to use leaden vessels for holding water as little as possible; for it was clear that health was in constant danger from it.*"

Lastly, there are perchance other chemical or electro-chemical causes, whose action is presently unknown or imperfectly understood. These I leave with confidence for investigation in the hands of the Chemical Section of the British Association; for on such causes or their action I am not prepared to throw any light.

I firmly believe that cases of lead poisoning on the small scale, or rather, I should say, in a minor degree, are constantly occurring in all our large towns from the plumbeous impregnation of drinking-waters. The medical man is often extremely puzzled to account for certain anomalous symptoms in his patients; and he puzzles himself in vain, until he at last bethinks himself of the assistance of the chemist, who discovers lead in the drinking-water used. Attacks of lead colic—isolated or endemic—are frequently the first circumstance to attract attention to the state of the water supply. On talking over the matter with medical men in London and elsewhere, I find the suspicion strong, though they are seldom in a position to prove indubitably the correctness of their suspicion, that many obscure cases of colic and other intestinal affections, as well as of paralysis of the nature of lead palsy,—sometimes going on to a fatal issue,—are really due to plumbeous impregnation of drinking-waters; and under so many different circumstances do such cases occur, that, in a large proportion at least, the water so impregnated must be *hard* in the sense in which I have used that term. In all cases of obscure colicky or paralytic complaints,—especially if it be found that several persons are simultaneously affected, —the condition of the water supply should be diligently inquired into, with a special view to the presence in it of lead. Dr Roberton of Manchester remarks,—"What I would here advert to is the custom, so general in England, of using lead cisterns for storing water, especially where, as in many of our towns, the supply is intermittent. Such cistern water is *always tainted with lead*, not in quantity, it may be, to produce the distinguishing symptoms of lead poisoning, but enough to affect very injuriously the stomach and nervous system of persons habitually using it."*

For the benefit of those who may be desirous of following up the subject of the foregoing remarks, I beg to append, in a collected form, a few bibliographical notes:—

Professor Christison of Edinburgh:
  1. Treatise on Poisons. 3d edit. 1835. Edinburgh.
  2. Dispensatory. 2d edit. Edinburgh, 1848.
  3. Paper in Transactions of Royal Society of Edinburgh. Vol. XV., Part II., 1842.

Professor Pareira:
  1. A Treatise on Food and Diet. London, 1843.
  2. Elements of Materia Medica and Therapeutics. 3d edit. London, 1849.

Professor Taylor:
  1. On Poisons. London, 1848.
  2. Guy's Hospital Reports. No. VI., O.S.

Dr Wall of Worcester: Transactions of London College of Physicians. II. 400.

Dr Scudamore: Analysis of the Water of Tunbridge Wells. 1816.

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* A Few Additional Suggestions with a view to the Improvement of Hospitals. By Mr John Brotherton. [Reprinted from the Transactions of the Manchester Statistical Society. Read March 31, 1858.]

Dr Thomson of Glasgow in Appendix to above. Also in Edinburgh Medical and Surgical Journal, Vol. XII.
Dr Lambe of Warwick: Researches into the Properties of Spring Waters. 1803.
Dr Yeats: Hints on a Mode of Procuring Soft Water at Tunbridge. Journal of Science, XIV. 352.
Vitruvius: De Architectura. L. VIII., c. 7. Quot modis ducantur aquæ. Ed. Dan. Barbari, 1567, pp. 262–5.
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Professor Graham: Elements of Chemistry. 1842: and Vol. II. London, 1858.
R. Phillips, in Report from the Select Committee of the House of Lords appointed to inquire into the Supply of Water to the Metropolis. 1840.
Dr Bostock, in Report of the Commissioners appointed to inquire into the state of the Supply of Water in the Metropolis. 1828.
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Dr Medlock: "On the Action of certain Waters upon Lead:" Record of Pharmacy and Therapeutics of General Apothecaries' Company, London. Part 2. 1857.

Descriptions of some new species and varieties of Naviculæ, &c., observed in Californian Guano. By R. K. GREVILLE, LL.D., F.R.S.E., &c.

Among the *Naviculæ* I have observed in Californian Guano are several of a very perplexing appearance; especially those belonging to the group of which *N. Lyra* may be considered the representative. Some of these forms strikingly illustrate the exceeding difficulty of determining what characters really go to constitute a species among these minute and variable plants. But it is amusing to notice the difference of opinion which still exists on the question of species. Professor J. G.

Agardh, after quoting some eminent naturalists, thus briefly recapitulates:—"Ex his, quæ breviter attulimus, satis, credo, apparet, tres nostræ ætatis vel excellentissimos naturæ investigatores in illa, quam proposuimus, quæstione dijudicanda, inter se dissentire. Schleidenius sola individua, Lindley species, Friesius species et genera a natura vult constituta, majores omnes ordines ab arte inventos esse."*

We have unquestionably much to learn regarding the value of characters presented by the Diatomaceæ. In the present state of our knowledge, it would appear that scarcely any one character taken by itself is to be relied on; and that even a combination of characters which may be sufficient for the determination of species in one genus may be unsatisfactory in another; and where groups or sections happen to be, what is called exceedingly natural, the difficulty is greatly increased. Indeed, it often becomes a question, whether it is best to leave a doubtful variety to embarrass the diagnosis, or to separate it under a provisional character. No law can be laid down on this subject which shall practically be a clear and unerring guide. Among the Diatomaceæ, the process of self-division, by means of which any deviation from the normal condition of a species becomes stereotyped and perpetuated with inconceivable rapidity, complicates the idea of a species to an extent unknown among the higher orders of vegetables. For example, let A represent a species of Diatom. By some unknown cause, one of its progeny, B, becomes so changed as to constitute a well-marked variety. Another of its progeny, C, undergoes a different but equally decided change; and possibly the same thing may occur in others. Now, these varieties or aberrations from the typical condition may be propagated, according to the late Professor Smith's calculation, at the rate of a thousand millions in a single month. Then, as there is no reason why B and C should not also have an indefinite number of nonconformist children, all removed in one character or another a *second stage* from the type, and producing duplicates by thousands of millions, it is manifestly impossible to say where the confusion is to end. But this is not all. By the process of conjugation, what Mr Thwaites calls "Sporangial frustules," are produced, which are very much larger than the ordinary size of the parents, and these it is presumed, multiply equally freely by self-division, and are equally liable, from accidental causes, to have their deviations from the normal state perpetuated. Such is the theory; and to arrive at anything like fixed specific distinctions would seem to be almost a hopeless endeavour. Nevertheless, by correcting processes unknown to us, we cannot doubt that the typical characters of real species are preserved. There must be a limit to the influence of the disturbing causes above mentioned; for order and individuality are conspicuous in the marvellous works of the Great and All-wise Creator.

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* Theoria Systematis Plantarum, &c., 1858, a learned and valuable work.

NAVICULA, Bory.

N. irrorata, n. sp. Grev. Valve oblong, suddenly contracted at the obtuse and produced extremities; striæ slender, conspicuously moniliform, interrupted, forming a broad band parallel with the margin, and a very narrow and irregular one next the median line; the linear blank spaces disappearing before reaching the ends. Length ·0055″ to ·0070″; breadth ·0020″ to ·0024″; striæ 15 in ·001″. (Pl. IV., fig. 1.)

In Californian Guano.

A remarkable species, having considerable affinity with *N. prætexta* (Ehr.), figured in Professor Gregory's Paper on the Diatomaceæ of the Clyde (Trans. Roy. Soc. Edin., vol. 21). The striæ in *N. irrorata* are slender, and, under a sufficiently magnifying power, are found to be composed of distinct oval granules about 15 in ·001″. The marginal band of striæ constitutes about a third of the entire breadth of the valve; the inner edge of the band forming a straight line until near the extremities, where it curves inwards. Opposite the nodule several of the striæ are finer and more crowded. Next the median line they are curiously irregular, influencing to a corresponding extent the contour of the blank spaces. At the nodule these striæ consist of three or four granules; at the distance of about a third of the space between the nodule and the extremity, the granules are rather suddenly reduced to one; then after a similar interval, they are increased to two, until they nearly reach the produced ends, which are filled up with granules more or less closely and irregularly disposed.

N. polysticta, n. sp., Grev. Valve elliptical, obtuse; striæ forming a narrow external band, moniliform, passing into distinct punctiform granules, which are scattered over the whole vacant area of the valve. Length, ·0024″; breadth, ·0011″; striæ, 25 in ·001″. (Pl. IV., fig. 2.)

In Californian Guano.

A minute but apparently well-marked species, allied to the preceding and to *N. prætexta*. The outline is purely elliptical; the marginal band of striæ less than the fourth part of the width of the entire valve. The space thus left unoccupied by true striæ is filled up by irregularly scattered puncta.

N. Lyra, Ehr., var. *recta*, Grev. Valve elliptical, obtuse; blank spaces contracted at the nodule, but otherwise parallel throughout their length with the median line, from which they are separated by a very narrow band. Length, ·0084″; breadth, ·0026″; striæ, 24 in ·001″. (Pl. IV., fig. 3.)

In Californian Guano.

I offer this truly splendid *Navicula* as a variety of the protean *N. Lyra*, the precise characters of which species we are not, I apprehend, even yet in a position to define. I have only seen two examples of the extraordinary size I have represented, which may be sporangial frustules, but have noticed many approaching towards it, all more or less elliptical or slightly produced at the ends, and distinguished by the straight narrow blank spaces.

N. approximata, n. sp., Grev. Valve oblong, obtuse, produced at the ends; striæ interrupted; the linear acuminated blank spaces separated from the median line by an extremely narrow band, straight opposite the nodule, somewhat converging towards the extremities. Length about ·0044″; breadth about ·0018″; striæ, 17 in ·001″. (Pl. IV., fig. 4.)

In Californian Guano.

Although belonging to the same group, and allied to *N. Lyra*, this seems to be really distinct. I have examined many frustules, and they uniformly agree in the total absence of any contraction of the blank spaces opposite the nodule.

On the contrary, there is frequently a tendency to become slightly convex at that part, as is seen in the figure I have given. From *N. Hennedyi* it differs in the form, in the linear blank spaces being closely parallel with the median line, and in the much larger proportion of blank space round the nodule in consequence of the greater shortness of the extremely narrow bands of striæ next the median line. The striæ are also less numerous than in both *N. Lyra* and *N. Hennedyi.*

N. Californica, n. sp., Grev. Valve broadly elliptical, obtuse, the sides somewhat angular; striæ moniliform, interrupted, constituting a narrow marginal band, and a very narrow row on each side the median line, the larger portion of the valve being blank space. Length, ·0044″; breadth, ·0027″; marginal striæ, 20 in ·001″. (Pl. IV., fig. 5.)

In Californian and South African Guano.

A very fine, and apparently well-marked species. A larger series of specimens might exhibit the same tendency to variation in outline that is so frequent in the section of *Naviculæ* furnished with interrupted striæ. In an individual from South African Guano now before me, the frustule is somewhat narrower, but in other respects precisely the same, even to the sides of the valve, which, being straight for a short space in the middle, produces an angular appearance. The singularly large proportion of blank space at once arrests the eye, and in contrast with it the exceedingly narrow row or band of striæ close to the median line. It may be remarked that these striæ, which are usually assumed to be the continuation of the marginal striæ, interrupted only by the intervening blank space, in this case take an unexpected direction. The marginal striæ throughout their whole length are arranged so as to form lines (were they continued) concentric with the extremities; but near the ends, the short striæ next the median line, instead of being pointed so as to meet the others concentrically, are directed so as to form an acute angle with them, and actually do so where the two sets of striæ meet.

N. nummularia, n. sp., Grev. Valve suborbicular; striæ moniliform, interrupted by two narrow linear blank spaces which contract opposite the nodule, and then curve outwards and converge and meet at the terminal nodules. Length, ·0012″ to ·0018″; breadth, ·0010″ to ·0015″; striæ about 24 in ·001″. (Pl. IV., fig. 6.)

In Californian Guano.

A curious little species belonging to the *N. Lyra* group, with the striæ highly concentric with the extremities. The blank spaces have a considerable resemblance to those of *N. forcipata.* Were it not for these blank spaces the frustules might easily be referred to *Cocconeis.* This may prove to be an extreme form of some other species.

N. gemmata, n. sp., Grev. Valve linear oblong, obtuse, the sides straight or slightly concave; striæ moniliform, interrupted, reaching half-way to the median line, with a single row of puncta intermediate between the striæ and the line. Length about ·0058″; breadth about ·0016″; striæ, 10 in ·001″. (Pl. IV., fig. 7.)

In Californian Guano.

An exceedingly brilliant and beautiful diatom, well distinguished by the distant striæ, which form a band parallel with the side of the valve, and gradually narrowing at the ends. Two rows of puncta (one on each side) are situated half-way between the striæ and the median line, which diminish in size as they approach the nodule and the extremities. The affinities of *N. gemmata* is with *N. Crabro* and its allies.

STAURONEIS, Ehr.

S. apiculata, n. sp., Grev. Valve elliptical oval, obtusely apiculate; stauros linear, reaching half-way from the median line to the margin. Length, ·0020″; breadth, ·0010″; striæ fine, 34 in ·001″. (Pl. IV., fig. 8.)

In Californian Guano.

A graceful, but inconspicuous object, not liable to be confounded with any other species known to me.

Explanation of Plate.

PLATE IV.

Fig. 1. Navicula irrorata; 2. N. polysticta; 3. N. Lyra, *var.* recta; 4. N. approximata; 5. N. Californica; 6. N. nummularia; 7. N. gemmata; 8. Stauroneis apiculata.

On some Fossil Bovine remains found in Britain. By Wm. Turner, M.B., London; Demonstrator of Anatomy, University of Edinburgh.*

The Fossil remains which I am about to bring before the notice of the Society this evening belong to the Bovine Family of the order Ruminantia. They have been collected from various localities, and have been placed at my disposal for purposes of description by several friends to whose care their preservation is due, and to whom I must confess my acknowledgments for permission to make use of them on this occasion.

The largest and most characteristic of these Fossil Bones are from the Anatomical Museum of the University of Edinburgh, where they have formed a part of the osseous collection for upwards of forty years. No description of them has ever been put on record. I have, through the kindness of Professor Goodsir, an opportunity of describing them to the Society this evening. These bones consist of two crania, a femur, scapula, humerus, the second cervical vertebra, a rib, and the left horn-core with a small portion of the frontal bone. Unfortunately no account either of the locality in which they were obtained, or of the deposit in which they were found lying, has been preserved; and from the length of time which has elapsed since they were discovered, it is almost hopeless to expect that any accurate information respecting these important and interesting particulars will ever be obtained. If one might form an opinion, however, respecting the nature of the deposit in which they had been imbedded, by the deep brown colour of the outer surface of all these bones, with the exception of the larger cranium, one would be led to suppose that they had reposed for a lengthened series of years either in close contact with a peaty soil, or in water deeply impregnated with organic matter.

The two crania present all the characters which belong to that great fossil ox which has been described by Bojanus and Owen by the name of *Bos primigenius*. These are especially the great length and peculiar curvature of the horn-cores, their origin, from the extremities of the ridge which separates the frontal from the occipital portions of the cranium, and the slightly concave forehead with which the plane of the occiput forms an acute angle; characters which were first specifically laid down by Cuvier,* and which enable the anatomist to distinguish the cranium of the Bos from that of the Aurochs, with which, at the first glance, it might be confounded. Both the crania have evidently belonged to fully formed animals, probably, indeed, advanced in years; for the sutures are almost without exception obliterated by ossification, and the two last posterior molars which remain on the left side of the upper maxilla of the best preserved skull are very much worn. These are the only teeth which have been preserved; portions of the fangs of one of the other molars are still present, however, in their sockets. In this skull, all the bones of the cranium, as well as those of the face, with the exception of the lower jaw, are in an almost perfect condition. The other cranium is not in so good a state, for all the facial bones have been broken away, and the base of the skull is very much injured; the ends of the horn-cores have also been broken off, so that their original length cannot be ascertained. In both crania the horn-cores are tuberculated at the base, and marked with long grooves on the surface. These characters are also seen in the single detached horn-core. Both crania exhibit corresponding dimensions in their several parts, so far as they are present in the two specimens, so that one must suppose that the animals to which they belonged were of equal size in most particulars. I subjoin some of the principal measurements, contrasting them at the same time with those of "the magnificent specimen of an entire skull from near Athol, Perthshire, now in the British Museum." †

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* Read before the Royal Physical Society, 23d February 1859.

	British Museum. Inches.	E. U. Museum. Inches.
Length of Skull,	36	26½
Breadth of Forehead between horns,	10½	9½
Breadth of Occipital Condyles,	6	6
Length of series of Upper Molar Teeth,	6½	6½

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* Menagerie du Museum d'Hist. Nat. Art. du Zebu; *Ossemens Fossiles*, t. iv. p. 109.

† Owen—"History of British Fossil Mammals and Birds," p. 501.

Whilst the skulls agree pretty closely in several of their measurements, there appears to be a considerable difference between them as regards their length. Professor Owen does not state what were the extreme points between which he measured. In the most perfect of the two crania in the Edinburgh University Museum, the length of which is above given, the line was drawn from the frontal ridge between the horn-cores to the tip of the intermaxillary bone. If the line be extended backwards, however, as far as the occipital foramen, the measurement is increased to 33½ inches. The span between the tips of the horn-cores is thirty inches, and the circumference of the cores at the base 15 inches; between the orbits the skull measures 12 inches.

If, as may very reasonably be conjectured, these crania were found in Scotland, they add another to the many previously existing examples of the existence of this large Bos in this country. In addition to the instance already quoted from Professor Owen's work, Dr Fleming* has recorded instances of the existence of large bovine crania in the marl-pits of Scotland, "exhibiting dimensions superior to those of the largest domesticated breed." These, however, he refers to the *Bos taurus*. I have had an opportunity of seeing the cranium particularly referred to by Dr Fleming. It is a remarkably fine example of the *Bos primigenius*, the teeth, especially, being well preserved. It is now in the Museum of the Free Church College. In the Museum of the College of Surgeons in this city, is a well-marked specimen of the cranium of this animal. In the Museum of the Society of Antiquaries there are three crania, two of which are in very good condition, they were obtained in the southern counties of Scotland. Dr J. A. Smith informs me that about forty years ago many crania were discovered in these counties by the diggers for marl.

Mr Parkinson also, in his "Organic Remains,"* states that he has in his possession specimens of Bovine Fossils obtained in Dumfriesshire. Professor Owen is of opinion that the *Bos primigenius* "maintained its ground longest in Scotland before its final extinction." This opinion is founded on the very recent character of the osseous substance.

* History of British Animals, p. 24. † Vol. iii.

It is probable that the femur, scapula, humerus, rib and vertebra were found in the same deposit, and along with the least perfect of the above described crania; for they present in their deep brown colour corresponding appearances externally. If such is the case, we shall be justified in regarding them as bones belonging to the *Bos primigenius*. On contrasting them with the corresponding bones of the modern Bos inhabiting this country, I find that they present the same anatomical characters, only on a much larger scale, on account of their greater size. This is seen especially in the spines, trochanters, tuberosities, and other ridges and prominences for the attachment of muscles and ligaments, which are all developed in an exaggerated form, and indicate most prominently what the great muscular development of the animal must have been. These bones are all in an admirable state of preservation, the osseous characters being distinctly marked, and the various articular surfaces smooth, and presenting their divisions into distinct facets as clearly as in the recent bones. As a means of arriving at a proper estimate of the great size of these bones when contrasted with those of the common ox, I subjoin certain comparative measurements which I have made, premising that the bones of the common ox which I have taken, although obtained from a young specimen in which the epiphyses are only partially united to the shafts by ossification, have yet, from their size, evidently belonged to an animal of a large breed. The fossil bones, on the other hand, have all belonged to an adult animal, for the epiphyses are completely ossified to the shafts.

Right Femur.	Fossil. Inches.	Existing. Inches.
Extreme length,	20¾	17
Circumference of middle of shaft,	7¼	5¾
Diameter across condyles posteriorly,	6	4¾
Greatest diameter of upper end of bone,	7	6
Left Scapula.		
Extreme length,	19	15¼
Extreme breadth,	10¼	9¼
Length of spine,	15	13
Longest diameter of glenoid fossa,	3¼	2¾
Right Humerus.		
Extreme length,	17½	13

Right Humerus.	Fossil. Inches.	Existing. Inches.
Circumference of middle of shaft,	9½	7
Breadth across condyles,	4¼	3½
Greatest diameter of articular surface of head,	5	4

The rib is most probably the seventh on the right side. Its length is 28 inches, and its greatest breadth 2½ inches.

The vertebra is the second cervical or axis.

	Inches.
Extreme height,	8
Extreme antero-posterior diameter,	7
Circumference of anterior articular surface,	14

The bulky nature of the spine and other processes of this vertebra point to the great size of the head of the animal to which it belonged, and indicate a corresponding development of the muscles and ligamentous structures which must have been connected to it.

From some further measurements that I have made, I have endeavoured to arrive at some estimate of the size of the entire skeleton of this great extinct Bos; this has been done by comparing the length of certain of the bones of the common ox with the height of that animal's skeleton, and then contrasting them with corresponding bones of the fossil animal now before us; this comparison has led me to the conclusion, that the skeleton of the extinct animal must have stood nearly six feet in height at the shoulder. If we now imagine this skeleton clothed with a thick coating of powerful muscles and hairy integument, having appended to the anterior extremity of the spine an enormous head with a pair of large and widely curving horns, we may form some idea of the formidable appearance that this extinct animal must have presented when, in the full vigour of its existence, it roamed unfettered through its native forests.

The other fossil remains, of which I have specimens, were found at different periods in the north-western division of the county of Lancaster. They were not obtained in the same locality, but at places several miles apart from each other; some being found in the district of Pilling, in the immediate neighbourhood of the mouth of the river Wyre, others close to the town of Preston. From information with which I have been supplied, I think it probable that the stratum in which they were imbedded was of the same nature, consisting of sand and gravel, lying immediately beneath the peat.

The specimens from Pilling consist of a large vertebra and a tooth. They were transmitted to me by the Rev. J. D. Banister, the incumbent of that district, a gentleman who, for a long series of years, has paid great attention to the natural history of the locality, and who carefully preserves any object of interest which may fall in his path. I have been favoured by Mr Banister with the following particulars of the deposit in which they were found:—"Pilling Moss is an extensive post-tertiary fresh-water deposit, situated between the mouths of the rivers Wyre and Cocker. It forms the present coast-line between these rivers, and is bounded on the land side by an ancient sea-beach, distant, on an average, two miles from the present sea-beach. The surface consists for the most part of fine corn land, and beneath this the following layers may be observed:—

"1*st*, Grey bog moss, generally the growth of Sphagnum.

"2*d*, A darker and more solid bog, towards the bottom of which there is much wood.

"3*d*, *Carre*, or original soil, in which are the roots of the trees of the ancient forest. In this numerous ancient implements have been found 2½ feet deep.

"4*th*, Clay, varying in thickness from two to six feet.

"5*th*, Blue silt, or finely comminuted sand.

It is in the last of these deposits that the bone and tooth were found. In different parts of the same layer numerous bones, horns, and teeth of the red deer have at various times been discovered."

The vertebra is the fifth cervical, and in its shapes and general anatomical characters, especially the concavo-convex nature of the articular surfaces of the centrum, and the foramina at the roots of the transverse processes, bears a closer resemblance to the cervical vertebræ of the larger members of the order Ruminantia than to those of any other mammalian order.

Thinking, in the first instance, that it might belong to the *Megaceros hibernicus*, I made a close comparison between it and the fifth cervical vertebra of the skeleton of that animal preserved in the Natural History Museum of the University of Edinburgh. It differs, however, in several of its measurements, more especially in the antero-posterior diameter, in which it is considerably shorter. On the whole, it may be said to possess a much more elegant shape than the vertebræ of the Megaceros. In its characters it corresponds much more closely to the fifth cervical vertebra of a bovine animal; and, from its size, it has probably belonged either to the *Bos primigenius*, or to the great fossil Aurochs, *Bison priscus*. In confirmation of this opinion, I have the high authority of Professor Owen, to whom I presented a cast of the vertebra some months ago. It may be interesting to contrast for a moment this vertebra with a human cervical vertebra, when the difference between the relative size of the neural canals and the bony processes, is at once apparent, the neural canal of the fossil bone being very little larger than the corresponding canal in the human vertebra, whilst the processes of the former are many times larger than those of the latter. The circumference of the fossil, measuring it around the tips of the processes, is 26½ inches. From the almost perfect state of the bone, it must have reposed quietly in the position in which it was found, and have been subjected there to very slight external influences.

The tooth found in the same stratum, and in the immediate neighbourhood of the vertebra, is the last pre-molar of the right side of the upper jaw of a bovine animal, probably the *Bos primigenius*. It has three fangs. The inner surface of the crown is convex, the outer concave and sinuous, the extremities projecting into considerable points. A crescentic enamel island lies in the centre of the tooth, the concavity of which is turned towards the sinuous outer surface of the crown, the extremities of the crescent project to the pointed extremities of the outer surface. At the first glance, it might appear as if this tooth were too small for the cranium of so large an animal as the great extinct Bos; but it was found, on trial, exactly to fill the empty socket of the corresponding tooth in the large cranium of this species in the University Museum, already described. On referring to the cranium which belonged to Dr Fleming, I found the corresponding pre-molar tooth still in its socket, and presenting exactly the same anatomical characters.

The bones which I have obtained from the neighbourhood of the town of Preston were found in the year 1836, by the workmen employed in digging the foundations for the piers of the railway bridge over the river Ribble. They were preserved by Mr Joseph Thornber, and by him presented to S. B. Worthington, Esq., the engineer to the Lancaster and Carlisle Railway, who deposited them in the Museum of the Lancaster Mechanics' Institution.

Mr Thornber, in a note which he has furnished me with, states that the bones were found in a stratum of gravel and sand beneath the peat. This stratum rested on a bed of new red sandstone. One of these bones is an undoubted relic of the *Bos primigenius.* It was found 26 feet 10 inches below the surface. It consists of the left frontal bone, with the horn-core still attached to it, and springing from the left extremity of the great posterior ridge. The bone has evidently belonged to a young animal, for it has separated from the adjoining bones along the lines of the different sutures, so that it could not have been permanently connected to them by ossification. Its dimensions, also, are much less than those of the corresponding bone in the adult crania, already described, its length being only nine inches, and the circumference of the root of the horn-core nine inches. The core is much less tuberculated, and not so strongly grooved as in the adult specimen. The other bones consist of a portion of the base of the cranium, a portion of a rib, and the left innominate bone. Of these, it is probable that the first-named only has belonged to a bovine animal. This consists of the basilar process, occipital condyles, and the remaining portion of the ring of bone which surrounds the foramen magnum. It is difficult to say with absolute certainty what animal the other bones had formed a part of. The rib scarcely appears to be bovine, for it is thicker and more massive; the ridge upon its outer surface is proportionally much stronger, and there is only a trace of a groove upon its inferior margin.

The innominate bone is much more slender than the pelvic bones of the existing species of Bos. On comparing it with the corresponding innominate bone in the skeleton of the Megaceros in the Natural History Museum, so many points of similarity were found between them, that I felt inclined to pronounce it to be the innominate bone of the great extinct Irish elk; but, on closer inquiry, I learnt that it is very doubtful if many of the bones in that skeleton are genuine bones of the Megaceros, no less an authority than Cuvier, in the fourth volume of the "Ossemens Fossiles," stating, in some critical remarks which he makes upon an engraving and measurements of the skeleton transmitted to him by the late Professor Jameson, "that it is doubtful whether the pelvis is well authenticated, seeing that it is not so altered as the rest of the bones." I find that the innominate bones have been painted dark brown, so as to give them the same colour as the other bones of the skeleton; and on scraping off the paint, a clear white osseous surface is seen beneath.

It is probable that this innominate bone belonged to an animal of the genus Equus, for both in size, shape, and osseous markings, it corresponds most closely with the left pelvic bone of the horse. If this be the case, it adds another to the localities, cited by Professor Owen, in which the remains of this animal have been discovered, and from having been found in the same stratum as, and in the immediate neighbourhood of, the frontal bone of the *Bos primigenius*, it establishes the coexistence of these different genera in our island.

On Glacier Action and Glacier Theories. By ALFRED WILLS, Fellow of University College, London, Barrister-at-Law.* From a pamphlet on the Ascent of Mont Blanc, &c., printed for private circulation only. 1858.

There is hardly a subject in the whole range of science more eminently calculated to arrest attention, and to excite interest, than the investigation of the phenomena and causes of glacier action. For, whether we regard those majestic accu-

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[* As this Journal contains all the papers on Forbes's Theory of Glaciers, the Editors have thought proper to print the *resumé* given by Mr Wills, which embraces the recent views on this important subject.—ED. *New Phil. Jour.*]

mulations of ice and snow in themselves, and as forming some of the most picturesque and the grandest objects in creation, or fix the mind upon the vast part which they play, and the vaster part which, in ages past, they have played, in the economy of the physical world; whether we contemplate them merely as the most striking features in the great panorama of the Alps, or the Himalayas, or as an important agent in securing to the interior of large continents regular and constant supplies of water, by means of the rivers which they feed, and which carry verdure and fertility into regions that would otherwise be but arid wastes, they are full of material for interesting speculation, to the lover of nature, the poet, and the philosopher, alike. Their phenomena are on a scale which cannot escape the notice of the most casual observer. Vast walls of granite boulders, built across the valleys, or along their sides—rivers arrested in their courses, and dammed up, so as to create great lakes—huge blocks of stone transported bodily from the loftiest summits to the lowest valleys—the solid earth wrinkled in front of the advancing mass, like a frail sheet of paper—the surface of the living rock rounded and polished, sometimes for miles together—such are the marks of their agency which meet the eye at every step, and which he who runs may read, though he may not understand. Nor is the eye the only sense to which they appeal. From morning till night, the glacier speaks with almost ceaseless utterance, now in the sharp report of an opening crevasse, like the crack of a rifle, now in the crash of the falling avalanche, like the roar of a hundred pieces of artillery. These indications of glacier activity are patent to every one; but for the philosopher, and the accurate and scientific observer, there are others, less obvious, but perhaps more instructive and significant. The tiny scratches on the polished rock—the light deposit of curved and concentric dirt-bands, which can only be seen at sunset from some neighbouring height—the delicate veins of granulated ice, which intersect the denser and more closely compacted structure of the general mass—the superposition of different layers of snow, belonging to different years, as seen in the bosom of a deep crevasse—these are specimens of the language in which they reveal their origin, their composition and their history to the philosophic mind of a De Saussure, an Agassiz, or a Forbes.

Considering the obvious and striking character of many of these phenomena, we are almost tempted to wonder, that it was so long before they attracted scientific attention; but our wonder ceases, when we reflect that the regions where alone these phenomena display themselves are remote and rugged, and that within little more than half a century, a journey to Chamouni was a scarcely less formidable undertaking than at present would be a journey to the wilds of Siberia, or to the icy wastes of Spitzbergen or Nova Zembla. De Saussure travelled among the valleys of the Alps with a retinue which would now suffice for a difficult exploring expedition in the Cordilleras, or the Rocky Mountains. In the northern valleys of Piedmont, and in the southern valleys of Switzerland, the more terrible apprehension of robbery and assassination was added to the awe inspired by natural obstacles and dangers. It is therefore not surprising, that the oldest and crudest glacier theory dates no farther back than the time of that great philosopher and naturalist, De Saussure.

My purpose is not to give any elaborate details as to the structure and movement of glaciers, but simply to attempt a short and popular account of the different theories which have been framed to explain the results observed, together with some examination of their respective merits. It is almost needless to say, that I make no claim to originality on behalf of this chapter, the contents of which are necessarily, in a great measure, abridged from Charpentier, Agassiz, Forbes, and other distinguished philosophers who have written on the subject of glaciers.

Reference has been made, in a former chapter, to the state of continual restlessness and change which characterizes a glacier; the following most animated and graphic picture of glacier life is drawn by Professor Forbes. Speaking of the Glacier de Miage, in the Allée Blanche, he says,—

"The fissures were numerous and large; . . . . so uneven, and at such angles, as often to leave nothing like a plain surface to the ice, but a series of unformed ridges, like the heaving of a sluggish mass struggling with intestine commotion, and tossing about over its surface, as if in sport, the stupendous blocks of granite which half choke its crevasses, and to which the traveller is often glad to cling, when the glacier itself yields him no farther passage. It is then that he surveys with astonishment the strange law of the ice-world, that stones, always falling, seem never to be absorbed—that, like the fable of Sisyphus reversed, the lumbering mass, ever falling, never arrives at the bottom, but seems urged by an unseen force still to ride on the highest pinnacles of the rugged surface. But let the pedestrian beware how he trusts to these huge masses, or considers them as stable. Yonder huge rock, which seems 'fixed as Snowdon,' and which interrupts his path along a narrow ridge of ice, having a gulf on either hand, is so nicely poised, 'obsequious to the gentlest touch,' that the fall of a pebble, or the pressure of a passing foot, will shove it into one or other abyss, and the chances are, may carry him along with it. Let him beware, too, how he treads on that gravelly bank, which seems to offer a rough and sure footing, for underneath there is sure to be the most pellucid ice; and a light footstep there, which might not disturb a rocking stone, is pregnant with danger. All is on the eve of motion. Let him sit awhile, as I did, on the moraine of Miage, and watch the silent energy of the ice and the sun. No animal ever passes, but yet the stillness of death is not there; the ice is cracking and straining onwards—the gravel slides over the bed to which it was frozen during the night, but now lubricated by the effect of sunshine. The fine sand detached loosens the gravel which it supported, the gravel, the little fragments, and the little fragments the great, till, after some preliminary noise, the thunder of clashing rocks is heard, which settle into the bottom of some crevasse, and all is again still."*

De Saussure, in his "Voyages dans les Alpes,"—one of the most delightful books of travels ever published—records a striking result of the gradual and progressive movement of a glacier, which, at the same time, afforded a conclusive proof of its continued activity during the winter season, during which period, it is worthy of remark, the motion was, for a length of time, believed entirely to cease.

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* Travels through the Alps of Savoy, pp. 198–9.

He writes thus:—"As the glacier and its environs were wholly covered with snow, when it pushed forward the earth accumulated in front of its icy mass, this earth, in crumbling down, fell upon the snow, and made evident the slightest movements of the glacier, which continued under my eyes during the whole time of my observations. But it is in summer that the greatest effects are seen to result from this pressure of the ice against bodies which oppose its descent. The following is an example:—In the month of July 1761, I was passing with my guide, Pierre Simon, under a very high glacier, to the west of the Glacier des Pélerins. I noticed a block of granite, of nearly cubical form, and more than forty feet each way, poised upon the débris at the foot of the glacier, and which had been deposited in this spot by the same glacier. 'Let us hasten on,' said Pierre to me, 'for the ice which abuts upon this rock might push it forward, and roll it on to us.' Scarcely had we passed, when it began to slip; it slid first gently enough over the débris which served for its base; then it fell upon its front face, then upon another face; gradually, it began to roll, and as the slope became more rapid, it began to take leaps, first small, but soon immense. At each bound, splinters, both of the block itself, and of the rocks upon which it fell, leaped into the air; these fragments rolled after it down the slope of the mountain, and so formed a torrent of rocks, great and small, which in their course crushed to pieces the top of a forest in which they finally stopped, after having, in a few moments, cleared a space of nearly half a league, with a noise and ravage which were astonishing."

The vast and irresistible force exerted by a glacier in its progress is sufficiently evinced, not only by such phenomena as the one thus vividly described, but by the huge dimensions of the blocks carried along upon its surface, and the amazing mass often accumulated in its moraines. The great glacier table, figured in the frontispiece to the work of Professor Forbes already quoted from, was 23 feet long, 17 feet wide, and 3 or 4 feet thick, and rested upon a pillar of beautifully veined ice, 13 feet high, and was so delicately poised that it was impossible to conjecture in which direction its fall would occur. Its contents may be estimated roughly at more than 40,000 cubic feet.

In following the course of a glacier, by ascending from its base to its source among the mountains, the traveller passes through several distinct tracts or regions, marked by features almost as characteristic as those which distinguish the several zones of vegetation, at different elevations on the sides of Cotopaxi or Chimborazo. The first or lowest part of his path lies over a rugged mass, whose inclination often amounts to 15° or 20°, consisting of innumerable lumps of ice, firmly compacted, and marked by crevasses, whose curvature and general disposition assume a certain degree of regularity. Throughout this region, the snow which falls during the winter is completely melted during the summer. As he ascends, the slope of the glacier becomes gradually less, diminishing to 6° or 10°, till he has reached an elevation of some 8000 feet, at which height the region commences termed by the French naturalists, *Névé*, and by the Germans, *Firn*; "where the surface of the glacier begins to be annually renewed by the unmelted accumulation of each winter." The crevasses now become more irregular, attain dimensions more formidable than in any other part, and are further distinguished by often exhibiting in their sides a decided stratification, the several layers corresponding to the yearly deposits of snow upon the surface. Lastly, at an elevation exceeding 9000 or 10,000 feet, the glacier, as well as the peaks and ledges abutting upon it, is covered with snow of dazzling brilliancy.

We thus trace a gradual change in the state of aggregation of the mass, from the highest part down to the lowest; a condition, namely, of progressive consolidation. While we thus see the snow which annually falls upon the higher slopes of the glacier gradually converted into strata of ice, and assimilated with the general mass, on the other hand we observe as constant a waste of the surface in the lower parts, and while, on the whole, the point to which a glacier descends remains nearly stationary, or as often recedes as advances, from year to year, the supply of fresh matter to its upper extremity, particularly during winter, is constant and unfailing.

These phenomena in themselves would afford, if not a positive proof, yet a strong presumption, that the comparison is a true one, by which glaciers have been so often likened to streams, and that the icy torrent, though its motion be utterly imperceptible to the naked eye, still presses forward with a constant, steady, and irresistible force. This fact, long unnoticed by naturalists, is now universally recognised.

The reality of glacier motion was first incontrovertibly established by the observation of the fact that large and conspicuous rocks, resting upon the surface of the ice, changed their position, with respect to landmarks upon the adjacent mountain sides, and by the otherwise inexplicable circumstance that the blocks of which moraines are composed often belong to geological formations occurring only in spots far distant, among the highest peaks which crown the summit of the glacier.

In the year 1827, M. Hugi, of Soleure, in order to prosecute some geological and meteorological researches upon the glaciers of Lauteraar and Finsteraar, erected a cabin of white granite on the moraine formed by their junction, near the foot of the rock called the Abschwung. His stay there appears not to have been sufficiently prolonged to force upon his observation any change in the position of his hut; but in 1839, M. Agassiz, upon repairing to the same spot, found it 4400 feet below its original position, and again in 1840, 200 feet below. Between 1827 and 1839, M. Hugi had himself revisited the station, and had left in a bottle, within the cabin, a slip of paper, stating that in 1830 he had found it some hundreds of feet below its original position, and that in 1836 he had measured the distance and found it to be 2028 feet.

In 1832, whilst pursuing his way to the Jardin, Professor Forbes found, on the Mer de Glace, the broken remains of a ladder, which, for various reasons, he concluded to be the one used, forty-four years before, by De Saussure, in his celebrated excursion to the Col du Géant. The ladder had probably been left at the foot of the Aiguille Noire, and after making due allowance for the curvature of the glacier along its channel, this gives a distance of about 13,000 feet, or a mean annual motion of about 300 feet.

To M. Agassiz is due the honour of having recorded the first exact and systematic examination of the question. In 1840, he erected, near to the remains of the cabin of M. Hugi, a hut, commonly known as the "Hôtel des Neufchâtelois," from which point observations were regularly made as to the progression of the ice.

The most detailed and exact results, however, are those obtained by Professor Forbes, who in the months of June, July, August, and September 1842, made a series of elaborate and careful experiments upon the Mer de Glace of Chamouni. These experiments extended not only to the question of the relative rapidity of the movement, at different seasons, and during the day and night, but to the comparative velocities of different parts of the same glacier—of the velocity at the source, as compared with that at the middle and lower extremity, and of the velocity at the centre, as compared with that at the sides. The total annual motion of the Mer de Glace was found to be about 480 feet, an estimate which may be fairly taken to represent also the average movement of other ice-streams. It was likewise demonstrated that the motion of the centre of the glacier is swifter than that of the sides, and that the velocity of the higher parts is exceeded by that of the lower.

The most remarkable and important general conclusions of fact, however, were the following: that "thawing weather and a wet state of the ice conduce to its advancement, while cold, whether sudden or prolonged, checks its progress;" and that there was a "general and simultaneous" connection between the amount of motion observed, and the mean monthly temperature at Geneva and the Great St Bernard, during corresponding periods—heat invariably accelerating, while cold as certainly retarded, the progression of the glacier. These two general deductions—which observation established beyond a doubt—are invaluable, as affording the means of testing the correctness of the two principal theories, which have been put forward to explain the phenomena of glacier motion.

Whatever theory be adopted, in regard to the precise force which urges a glacier through its channel, it is readily observed how the straining and contortion of the mass among the sinuosities of its bed necessarily produce the crevasses which constitute so striking a feature of glacier conformation, and how, as a result of the different states of consolidation of the successive additions to its bulk, combined with the variation in the velocity of each portion of the stream, there are exhibited, wherever a section of the ice can be obtained, either vertically or horizontally, indications of a decided and peculiar stratification—the bands or layers of ice being contorted into characteristic curves.

The first hypothesis proposed to explain these phenomena was that of De Saussure, known as the "Gravitation theory."

It represents a glacier as a body essentially rigid and inflexible, which slides along its channel, simply in virtue of its own weight.

There are, however, palpable and fatal objections to this view; for, if it were correct, it follows that any sudden increase in the inclination of its bed would be indicated by a proportionally sudden acceleration of the motion of the glacier; in fact, that the motion of any given point upon its surface would be irregular, instead of uniform. Moreover, in accordance with known mechanical laws, such a mass must increase continually in velocity, until it became, at length, a vast avalanche, more particularly in the case where the bed has an inclination of 20° or 30°.

After an interval of many years, another and more ingenious theory was propounded by Charpentier. Of this—the "Dilatation theory," as it is called, which has been more fully developed and warmly upheld by Agassiz—the following is an outline:—

The ice of glaciers is traversed, in every direction, by capillary fissures and air cavities; which, during the heat of the day, become filled with water, melted from the surface, which remains there, "ready to be converted into ice by parting with the very small portion of heat which it contains." During the night, this water freezes, and, in consequence of its expansion during congelation, the entire mass of the glacier undergoes a certain dilatation, the effect of which is to produce a forward motion of the body of the glacier, in the direction of least resistance.

It will be at once evident that this hypothesis is wholly incompatible with the fact conclusively established by Professor Forbes, that heat invariably accelerates, and cold as constantly retards, the progress of the glacier, no less than with the results of observations made by Agassiz himself on the temperature of the ice, for some depths below its surface, as registered by himself and his party, at their station on the Aar glacier. At a depth of seven or eight French feet below the surface, and downwards, the mercury never rose above the freezing point—any changes in the actual temperature of the ice being, in truth, entirely superficial and insignificant.

If it be true, then, that water, in becoming ice, parts with a very small quantity of heat, how is it enabled thus to remain in contact, during the day, with surfaces at and below the freezing point, without almost instantly losing that small degree of heat, and being frozen? But the fact is, that a pound of water at 32° Fahrenheit, in becoming a pound of ice at 32°, parts with a very *large* amount of heat. Every one has noticed the long period often required for the gradual liquefaction of a large mass of ice or snow; and the vast supply of heat thus poured in, to produce the fluid state, is not lost or dissipated, but becomes "latent" in its mass. If the melted water be then frozen once more, this large quantity of heat is again given out, and becomes appreciable, so that the degree of cold necessary to produce congelation in a given weight of water is measured by the amount of heat required to melt the same weight of ice. Experiment, however, has shown that, in the congelation of water, as much heat is given out as would raise its temperature, if it could be so applied, by 142° Fahrenheit.

There is, therefore, no reason to believe that the water infiltrated into the capillary fissures of the glacier is subject to periodical congelation, in the manner assumed by this theory —at any rate, to such an extent as to be capable of producing the motion observed. The relative velocities at different times, and in various parts of the glacier, are also at variance with the requirements of this hypothesis.

A later attempt to connect glacier motion with some general principle adequate to explain its peculiarities is to be found in the "Proceedings of the Royal Society" for 1855, p. 333. It relies mainly upon the ordinary law of expansion among solids. Mr Moseley, the author of this suggestion, quotes, as an apt illustration, an instance in which a large sheet of lead, on the roof of Bristol Cathedral, by its alternate expansion and contraction, drew the fastenings out of the beams, and descended bodily, in the course of two years, a distance of about eighteen inches, towards the lower edge of the roof.

The theory, however, wholly fails to explain the general progression of glacier streams, inasmuch as the action attributable to expansion can be, at most, wholly superficial; nor does it seem adequate to account for any of those distinctive peculiarities inherent to the structure of glacier ice.

It remains only to notice the theory proposed by Professor Forbes, to whose precise, mathematically accurate experiments allusion has been so often made. It is an hypothesis which explains so consistently every fact in the history and phenomena of glaciers, as well in its minutest details, as in its broadest features, and is, at the same time, so admirably simple, as almost to have lost its speculative character, and taken its stand among geological certainties.

It is thus enunciated:—"A glacier is an imperfect fluid, or a viscous body, and is urged down slopes of a certain inclination, by the mutual pressure of its parts."

Now, upon reading this definition, the mind is involuntarily startled by the description of a glacier as a semi-fluid body; ice, in the masses in which we are accustomed to see it, appears so devoid of plasticity, that the conception of its viscosity presents, undoubtedly, at first sight, a formidable difficulty. We must, however, bear in mind, that we observe among some bodies, such as tar or plaster, every conceivable degree of cohesion, from that of almost perfect fluidity to that of solidity, without being able to draw a distinct line of demarcation between the several grades through which they pass. Stockholm pitch has been proved ("Phil. Mag." for April 1845) to move with extreme slowness under its own weight, when so far solid as to break into fragments under the blows of a hammer.

Every one is familiar with the elasticity, often considerable, exhibited by the thin sheets of ice which cover our ponds and pools, in the winter, as they bend and swell beneath the passing weight of the skater; and the following experiment, devised by Mr Christie (the late secretary to the Royal Society), clearly demonstrates that "under great pressures, ice preserves a sufficient degree of moulding and self-adapting power to allow it to be acted upon as if it were a pasty mass."

A strong iron shell, with a small fuse-hole, was filled with water, and then exposed to severe cold. As the congelation of the mass proceeded, the ice inside was forced out through the aperture, in a narrow cylinder, gradually increasing in length, until all the water was solidified. "As we cannot doubt that an outer shell of ice is first formed, and then another within, the continued rise of the column through the fuse-hole must proceed from the squeezing of successive shells, concentrically formed, through the narrow orifice; and yet the protruded cylinder consists of entire, not of fragmentary, ice."

When we take into consideration the minuteness of the motion of glaciers, as compared with the entire length of their mass, it is now less difficult to conceive how the vast mutual pressure of their particles may produce a degree of viscosity, or semi-fluidity, actual, and sufficient to generate the phenomena observed, though inappreciable, and apparently disproved by, the evidence of the senses, and only to be discovered by minute, accurate, and philosophical observation.

The peculiarities of glacier motion exactly fulfil, in every particular, the conditions well-known to mathematicians as those of the flow of semi-fluid substances; such, for example, as the greater velocity in the centre than at the sides of the stream, and in the lower, than in the upper portions.

The curves observed in the stratification of glaciers are precisely similar to those exhibited in the structure of bodies admitted to be viscous while in motion. It occurred to Professor Forbes to imitate the movements of glaciers, in those of a substance capable of flowing with extreme slowness, and ultimately solidifying; for this purpose, he employed a mixture of glue and plaster of Paris, and by allowing alternate layers of this mixture, coloured differently, to flow down a slightly inclined plane—a mimic glacier channel—obtained casts, presenting, in their sections, curves which resemble, in a very striking degree, those actually seen in the Swiss glaciers.

The experiment is easily verified, and the sections of the models thus formed, vertical to the direction of motion, will be seen to present the characteristic concave curves so conspicuous in several of the Swiss ice-streams, while those taken horizontally exhibit elongated curves, whose convexity is in the direction of motion, and the surface itself is traversed by crevasses in miniature, whose general disposition pretty accurately represents what is seen on the actual glacier.

This very close resemblance between the structure of glaciers and that of bodies undoubtedly semi-fluid or viscous in their character, while in gradual motion, affords, at least, a very strong presumption that the same mechanical conditions which produce the phenomena of the latter prevail also in the case of the former; it is, in fact, not only a beautiful illustration, but a pointed and decisive confirmation of the theory.

A theory of glacier motion has lately been propounded by Dr Tyndall and Mr Huxley, which is intimately connected with the phenomenon known as the "veined structure" of glacier ice. It becomes necessary, therefore, to explain concisely the nature of this structure, and of the hypothesis by which it has been sought to account for its formation.

Wherever the structure of compacted ice is displayed, as in the vertical walls of a crevasse, it is found to consist of alternate bands of blue and white ice lying side by side in parallel plates or laminæ, presenting different degrees of hardness. One of the great problems in glacier science is to account satisfactorily for these alternating bands of varying density and colour. The theory originally suggested by Professor Forbes for its explanation was, that the unequal motion of different parts of a glacier—which is most rapid in the centre, and least rapid where it is in close contiguity to the sides, and affected, therefore, by friction—causes "a solution of continuity between the adjacent particles of ice to enable the middle to move faster than the sides." Hence innumerable fissures are formed between the different slices, so to speak, which move on, side by side, with varying velocities. These fissures are filled with the superficial drainage of the glacier, and in time are frozen up, and thus "produce the appearance of bands traversing the general mass of ice having a different texture." The banded structure, however, is found not only at and near the sides, where a considerable amount of differential motion exists, but also in the centre of the glacier, where no such tension takes place. Here, therefore, Professor Forbes supposed that, the lateral friction being of little effect, another force comes prominently into action; and that, as there must be a great pressure from behind, owing to the weight of the upper part of the glacier, and as the body is of very imperfect fluidity, the resistance of the mass in front to the pressure behind, uninfluenced by the lateral friction, causes the particles of moving ice to "slide upwards and forwards over the particles immediately in advance." Hence the differential motion will be in a different direction from what it is at the sides. The planes of separation will be across the axis of the glacier, instead of being nearly parallel to it, and will have a *dip* towards the horizon, varying with the amount of resistance in front, and the amount of pressure from behind—varying, that is, according to the respective distances from the end and from the origin of the glacier.

Subsequent investigation, however, led Professor Forbes to the belief that the veined structure is the effect of "pressure upon the loose and porous structure of the snow." (Thirteenth letter on Glaciers, December 21, 1846.) As the different portions of the descending mass move onwards with varying velocity, subjected to the enormous compression which is the starting-point of every theory, some parts will be kneaded or worked against others, and by this means, snow will be converted into ice, in parallel bands corresponding with the direction of the planes of separation. A familiar instance of the conversion of snow into ice under pressure is afforded when boys make a slide upon snow. In a short time, the loose, uncompacted snow becomes a sheet of hard and glassy ice.

Dr Tyndall, when he first brought forward his views, would seem not to have been aware of this later opinion of Professor Forbes; for in a lecture upon the theory of glacier motion, delivered at the Royal Institution, January 23d 1857, he made no mention of it, and devoted a considerable part of the lecture to certain objections which he urged to Professor Forbes's original explanation of the veined structure by the congelation of infiltrated water. During an examination of glacier ice made by Mr Huxley and himself, in the summer of 1856, they discovered, it was stated, distributed throughout the mass of the glacier, long and narrow lenticular cavities filled with clear blue ice,* such as ice forms the *blue* bands of the veined structure, and which was at one time supposed by Professor Forbes to be the product of the congelation of the infiltrated drainage-water. It was not expressly stated, but it would seem from the diagrams referred to, as well as from the nature of the argument, that these masses are found in a direction of general parallelism to the direction of motion of the glacier. According to Dr Tyndall, they vary very much in size. One observed by him and Mr Huxley was two feet long by two inches broad, others two inches long by a fraction of an inch in breadth; and one measured no less than ten feet long. How, he argued, could motion such as that suggested by the viscous theory have produced these lenticular cavities in the middle of the glacier? Had there been the suggested thrust from behind, and resistance in front, they would certainly have been closed up. Secondly, says Dr Tyndall, if the explanation given by Professor Forbes of the formation of the blue bands be correct, the fissures, in which the reservoirs of water which make the blue bands are formed, must be of equal thickness; and before the water in them is frozen, they should be found filled with clear blue water. The blue bands vary from a fraction of an inch to many inches in thickness, and therefore such fissures could scarcely escape observation if they existed. But they have never yet been found, and may therefore be assumed not to exist. According to Professor Forbes himself, the freezing takes place chiefly, if not exclusively, in winter; so that, throughout the summer, the matrices of these blue bands should present themselves as narrow reservoirs of clear blue water—a phenomenon which no observer has ever yet discovered.

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* Mr John Ball, in a valuable paper contributed to the "Lond., Edin., and Dublin Phil. Mag." (Dec. 1857, Vol. XIV. p. 493), takes a just exception to the description given by Dr Tyndall and Mr Huxley of the "Lenticular Structure" of glacier ice. "So long," he says, "as the new expression was confined to the particular and unusual condition of the ice, no objection could be made to it; but if I am not under a grievous misconception in believing that the blue veins may usually be traced for a distance of many yards, and almost constantly for several feet, I may be permitted to appeal to the subsequent and wider experience of the authors of this new term against the use of it as generally decriptive of the phenomenon to which they seem disposed to apply it."

Such is an outline of Dr Tyndall's objections to the theory which accounts for the blue veins by the freezing of the glacier-drainage. As between him and Professor Forbes, however, the discussion of these objections seems hardly called for, since Professor Forbes recanted, so to speak, in 1846, and published his own corrected view, ascribing the veined structure to pressure. In an elaborate article contributed to the "Westminster Review" (April 1857), by a writer deeply imbued with the views of Dr Tyndall and Mr Huxley, and in which those views are warmly advocated, it is very properly remarked that the change in Professor Forbes's views renders it unnecessary to inquire whether his original opinion was correct or not.

Dr Tyndall discards altogether the notion that ice is a viscous or plastic body. Wherever, he says, the banded structure displays itself, the ice is found to follow a law discovered by himself, and which is applicable to every substance in nature not strictly homogeneous. When subjected to and consolidated by pressure, the phenomenon of *cleavage*, or the property of being capable of lamination in definite and parallel planes, is exhibited. Take a piece of slate rock, pound it into powder, mix it up with water into its original mud, subject it to pressure, and you reproduce the original slate, capable of being split into parallel tables equally with a slab fresh from the quarry. Wherever the banded structure is found, the property of cleavage exists. The comparison, which Professor Forbes himself suggested, to slaty cleavage is, according to Dr Tyndall, no mere analogy. The planes of cleavage in the ice, as in the slate, are always found to be perpendicular to the direction of greatest pressure—a fact which was suspected, though not established, by Professor Forbes himself. Dr Tyndall considers that the lenticular masses observed in the glacier ice are analogous to the blue and green lenticular masses which occur in common slate, and present themselves to every schoolboy's eye. The similarity of form suggests a common origin, and they are probably due to analogous causes, whatever those be, in each case. The laminated structure, therefore, according to Dr Tyndall, is not due to differential motion, but is the genuine effect of pressure acting in a direction perpendicular to the structure.

But ice, it will be said, must be plastic, to reunite under pressure, as it does in the course of its descent. Mr Tyndall thinks this phenomenon strictly in accordance with the common and obvious properties of ice; and he exhibited at the Royal Institution, and has since, in his published papers, referred to a number of interesting experiments, in which lumps of ice were put between moulds of various forms, and, being subjected to severe pressure, were broken and crushed, and the fragments squeezed together again into solid bodies of a totally different shape. This phenomenon he attributes, not to viscosity, but to the property which was announced by Dr Faraday in 1850, and which Dr Hooker has named "regelation"—in virtue of which two pieces of ice at 32°, being subjected to pressure, will be frozen together, and unite by a series of slender icicles or columns of ice, running into one another, so as to form one solid mass. That this was the result of some distinct and independent property, and not merely, as had been once supposed, the effect of evaporation upon the wet surface, was shown by the fact that the process takes place with equal certainty under water, and even under boiling water. It is in virtue of this principle, according to Dr Tyndall, that snow becomes converted into ice, at depths below the surface at which the effects of external temperature are inappreciable. It is in virtue of this principle that ice, broken into fragments by pouring over a ridge in its bed, or by being precipitated over the edge of a precipice, is reconstructed on a gentler slope, or at the foot of the rock from which it fell, and the glacier flow is continued, or a fresh secondary glacier formed, as the case may be.

The chief merit of the views advanced by Dr Tyndall and Mr Huxley appears to me to consist in the greater distinctness and prominence which they give to the operation of pressure upon ice as producing cleavage—a subject which is alluded to and suggested, over and over again, in Professor Forbes's writings, but never pursued to the extent to which the later investigators have carried it. In other respects, it may well be doubted, I think, whether the arguments propounded by Dr Tyndall really touch the viscous theory at all. Dr Tyndall says ice is not viscous; the idea is contrary to all we know of its physical properties ; and he appeals to the experiments which have been mentioned, as conclusive proof that the change in the form of the blocks of ice takes place through fracture and regelation. With unfeigned diffidence, I am unable to see that this conclusion is fairly deducible from them, or that they go further than to show that some of the phenomena of that pressure, the existence of which all theorists alike take as their starting-point—to whatever causes they attribute it, or whatever may be the precise part they make it play in the glacier economy—may be exhibited upon hand specimens, as well as in the vast masses upon which nature displays her workings. If so, they really do not assist Dr Tyndall. Indeed, an argument may fairly be drawn from the completeness of the reunion of the crushed fragments in favour of the viscosity or semi-fluidity of ordinary ice. There is something very peculiar in the sound emitted by the ice when subjected to the pressure employed in these experiments. It is anything but the sharp short crack of most substances of undoubted brittleness; it is a protracted, creaking, hissing sound, indicative rather of yielding than of breaking, and strongly reminds one of the sound given by cork when violently squeezed.

Be this as it may, however, the substance of which a glacier is composed is very different from common ice. Professor Forbes considers it to be rather a mixture, or compound, of ice and water, than a mass of mere ice. This may well be the case even in the winter, and down to its lowest depths. Balmat has told me that upon two occasions, when crossing the Mer de Glace and the Glacier des Bossons in the middle of winter, he has struck his alpenstock through the covering of snow and found a pool of water underneath. At a very few feet below the surface, the temperature is probably very nearly uniform throughout the year. Dr Tyndall, in a late lecture at the Royal Institution, has shown that the phenomena of regelation *will not take place*, unless moisture be present amongst the particles of ice operated upon,* and every schoolboy knows that he cannot make a snowball, unless the snow is melting. Thus, then, the regelation theory affords a striking and unexpected confirmation of the fact that a glacier is a mixture of ice and water.

The name and the peculiar nature of regelation were not known when Professor Forbes wrote; but it is perfectly evident from his Thirteenth Letter, and from very many passages in his works, that he was aware of the effects of pressure (a condition of regelation), in producing the union of contiguous fragments, whether of snow or of disrupted ice.

The term "viscous" as applied to ice, has been strenuously objected to; but Professor Forbes's theory does not depend upon a word. It is, in truth, just what a theory should be—rather a compendious epitome of a multitude of observed facts, than a speculation. It will not be denied, in the present day, that certain facts have been established with regard to the motion of glaciers, which correspond in a remarkable manner with the observed facts relative to the motion of semi-fluid bodies. Even Dr Tyndall, though the zealous supporter of a new theory, has hardly questioned the accuracy of a single fact established by the laborious investigations, and the numerous and accurate measurements of Professor Forbes; and his lectures and published papers contain ample evidence that the numerous analogies between the motion of a glacier and that of a pasty fluid are recognised by him no less than by his opponents. In the lecture of the 23d January 1857, some of Professor Forbes's experiments upon the flow of semi-fluids were repeated, as illustrative of certain phenomena of glacier

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* I am disposed to think that when Professor Forbes, speaking of the slide, says the snow is converted into ice "by pressure and friction alone, without the slightest thaw," there is probably an error, and that the friction produces just thaw sufficient to enable regelation to take place. In arguing from the slide to the glacier, however, Professor Forbes points out the difference, and the greater readiness of the glacier snow to be transmuted, in consequence of the quantity of ice-cold water with which it is saturated.

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motion. Why draw an illustration from the flow of the mud, if the mud and the glacier have nothing in common? In the latest paper they have published upon the subject, Dr Tyndall and Mr Huxley have again recourse to the same class of illustrations, observing that "owing to the property of ice described in s. 3" (Regelation), "the resemblance between the motion of a substance like mud, and that of a glacier, is so great, that considerable insight regarding the deportment of the latter may be derived from a study of the former. From the manner in which mud yields when subjected to mechanical strain, we may infer the manner in which ice would be *solicited to yield* under the same circumstances." (Phil. Trans. 1857, p. 338.)

What is this but saying, in other words, that the consequences of the property of regelation are such that the phenomena of glacier motion and those of the motion of mud are very much alike? May it not turn out, either that regelation (or, if one may coin a word, *regelativeness*) is only another name for that particular kind of viscosity possessed by the ice of glaciers, or that the term carries us one step higher in the chain of causes and effects, and *points to the reason why glacier ice is viscous?* Why should it be instructive thus to observe the deportment of substances of undoubted viscosity, if there is no real analogy between their construction and that of a glacier—if the similarity be not the result of analogy but a mere accidental coincidence? The conclusion seems to be inevitable that Dr Tyndall, while objecting to the term *viscosity* as applied to ice, and having commenced the controversy by utterly denying the fact that glacier ice possesses properties analogous to those of a semi-fluid body, has been gradually forced, by a larger consideration of the question, and by more extended observation, to approach much nearer to the viscous theory than, at one time, he would have been willing to admit. Nor is it immaterial to observe that the views originally propounded by Dr Tyndall were not grounded, like those of Professor Forbes, on the accumulated experience of months and years spent amidst the ice-world. Dr Tyndall does not refer to any observations upon glaciers made by himself prior to 1856, and there is a remarkable note to the paper by Dr Tyndall and Mr Huxley, in the Philosophical Transactions for 1857, p. 334, in which they speak of the "Système Glacière" of Agassiz, as a work "which, until quite recently, they had not the opportunity of examining." The argument, from mere *authority*, however, in matters of science, ought to go but a very little way, and I should be very sorry to use it for more than it is worth. Science gains greatly by the candid discussion of questions of this nature, and there is no scientific subject upon which he touches, upon which Dr Tyndall is not likely to throw valuable light; but it is not unreasonable to say, that views formed upon a much less protracted consideration of the subject hardly possess quite the same claim to ready acceptance as those of Professor Forbes, whose labours in this field of inquiry have been extended over so many years—the duration of whose investigations may be measured by months, where that of Dr Tyndall's researches is counted in days, and the extraordinary care and accuracy of whose observations upon matters of fact have been impugned by no subsequent inquirer.

The effect of the discussions which have taken place upon the theories of glacier structure and motion will undoubtedly be to direct fresh attention to this most interesting subject, and, by a fresh series of experiments and investigations, to advance the great end of all scientific inquiry—the attainment of truth. At present, however, it is scarcely too much to say that the acute and ingenious papers of Dr Tyndall and Mr Huxley, though valuable contributions to glacier literature, have not endangered the position of the viscous theory.

An Account of Donati's Comet of 1858. By George P. Bond.*

The following account of the great comet, discovered by Donati on the 2d of June 1858, has been prepared for the "Mathematical Monthly," with the intention of furnishing correct information on a subject of universal interest, in language freed as far as possible from technical expressions. The popular character of the article renders a few preliminary words of explanation necessary in reference to the more distinctive phenomena presented in the motions and physical aspect of comets generally.

The first characteristic of these singular bodies is that of their being mainly, perhaps in most instances entirely, composed of an ill-defined gaseous or nebulous substance, endowed with properties so extraordinary, that it can scarcely be classed with matter, in the ordinary acceptation of the term. Of its extreme attenuation and lightness there can be no question. The planets, and among them our earth, must again and again have traversed unharmed the tails of comets. In October last, the debris of the magnificent train of the comet which has just disappeared from our western skies, swept over the region occupied by the earth a few weeks earlier. Instances of more immediate proximity are of too common occurrence to allow us to suppose that we are always to escape an actual collision; but it is inconceivable that any disastrous consequences could ensue to our earth or its inhabitants, any more than from contact with sunlight or with the ether of the planetary spaces.

A second chracteristic is that of internal condensation. All comets present this in a greater or less degree. Most of them have a minute stellar point, called the nucleus, which occupies the position of maximum density. There are others in which this latter feature is wholly wanting. But the number in which it cannot be detected with a powerful telescope is much smaller than has commonly been supposed. This centre of condensation or brightest point is, with rare exceptions, placed on the side which is nearest to the sun.

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* Reported from an Essay published in Boston, U.S., with omission of a few illustrations.

It is always, however, very close to the centre of gravity, as is proved by the fact of its motion about the sun, in accordance with the law of gravitation.

The nucleus itself is a minute point compared with the immense volume of light-giving substance, of which it is the controlling centre. Whether it is solid or not, is still undecided. As far as the eye alone is to be trusted, there are comets as truly solid as the planets or stars themselves. In size and weight, however, the true nuclei, apart from their surrounding nebulosity, are probably quite small, measured by the standard of the larger planets. Still it is possible that there may have been instances in which the mass of these bodies has been comparable with that of the earth, and yet they may have completed their circuit around the sun, leaving no appreciable trace of their disturbing influence—the only sure test by which their mass could be detected. The evidence, from the fact that the smaller stars shine freely even through the most condensed portions of comets, adduced by astronomical writers in proof of their transparency, and, by inference, of their extreme tenuity and lightness, has a certain value when applied to the class of feeble telescopic comets; but is scarcely applicable to one like that of the present year, which overpowered all but the brighter stars in the neighbourhood of the nucleus by its superior brilliancy.

The feature next in importance to the nucleus is the train, or tail, as it is usually called (although often preceding the nucleus in its motion), projected at an immense distance from it, and usually, although by no means invariably, in a direction opposite to that of the sun. The agency of the nucleus in the formation of the train, but still more in the subsequent control which it retains over it, is one of the most curious phenomena presented in nature. Often, several of these appendages are seen radiating at once from the same nucleus. The greatest variety in curvature of outline, length, brilliancy, and other peculiarities, is presented by different comets, or by the same one in different parts of its course. The portions near the axis are usually darker than the edges, giving at times the appearance of a division with a stream of light on either side.

The larger bodies of this class exhibit a wonderful complication of phenomena in the region contiguous to the nucleus. Of these, the most prominent are the interposition between the nucleus and the sun of one or more well-defined and rounded screens, or caps of dense nebulosity, called envelopes, partially but not entirely surrounding the nucleus, and the emission of streams or jets of luminosity, bright sectors, &c., in a direction inclined or opposite to that of the tail. With great variety of detail in other respects, these have all a well-marked tendency to appear in the first instance on the side of the nucleus next the sun. The great comet of the present year undoubtedly takes a foremost rank in respect of the multiplied and most curious changes which it has exhibited, and especially in the complete illustration which it has afforded of the origin, construction, and final dissipation of a succession of envelopes. In these phenomena, the process of the formation of the tail, from the substance in immediate contact with the nucleus, is intimately concerned. The astronomer, night by night, sees the work of evolution going on with an amazing rapidity, and seemingly in defiance of the best-established properties of matter, the laws of gravitation, and of inertia. The results are evident to all, but the secret cause is a profound mystery, admirably calculated to stimulate speculation and intelligent investigation.

The following figure (Plate V., fig. 1),* with a brief recapitulation, will serve to illustrate the leading phenomena presented in a telescopic view of a large comet; *n* represents the star-like nucleus, *a* and *b* the outlines of two envelopes on the side of the nucleus towards the sun, *c* the diffused exterior nebulosity which is a never-failing attendant.

The dark axis *d*, of the tail, is sometimes, however, absent, and again, at times, replaced by a bright ray. Ordinarily the convex and brightest side of the tail is presented to the region towards which the comet is moving.

As regards the motion of comets in space, it is a well-established fact, so far as our present means of observation extend, that their nuclei alone move in obedience to the attractive force of the sun and planets. This property, which has been recognised with consistency and uniformity, is not the least singular peculiarity of their constitution. Immense volumes of matter, apparently of the identical substance of the nucleus, go to compose the enveloping nebulosity and the tail; but from the moment of leaving the central body their motion is perfectly inexplicable, without assuming them to be under the influence of laws of force which greatly modify that of gravitation.

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* The engraving has been copied from a drawing of the comet of Donati made by the writer, at the Observatory of Havard College, October 2, 1858.

The shape of the cometary orbits, described about the sun, is nearly that of a parabola, or of an elongated ellipse, with periods of revolution varying from a few years to many centuries. The point in the orbit which is nearest the sun is called the perihelion ; the distance of this point from the sun, the perihelion distance, and the time of the comet's passing it, the perihelion passage.

Fig. 2 represents the form of a portion of the orbit of the great comet of the present year. The circles show the relative proportions of the orbits of Venus, the Earth, and Mars. The date, September 30, is the epoch of perihelion passage, the distance of the comet from the sun at that point being about fifty millions of miles.

For convenience of delineation the orbit is represented with its plane coincident with that of the earth's orbit. These planes have actually an inclination of sixty-three degrees to each other. The relative size and the form of the orbit, with the places of the comet in it, are given with sufficient exactness for the purpose of illustration. The arc described by the comet, during the period of its greatest brilliancy, is included between September 30 and October 10. It had, however, been detected by astronomers about three months earlier.

On the 2d of June 1858, a faint nebulosity, slowly advancing towards the north, was descried by Donati at Florence, near the star λ *Leonis*. This was the earliest observation of the great comet of 1858. Its place at the time is shown in fig. 2, its distance from the sun being then about two hundred millions of miles, while from the earth it was yet more remote. Being, at first, inclined to question whether it might not be identical with another comet just before seen in the same quarter of the heavens (the third comet of 1853), he communicated the intelligence of the discovery with a suitable reserve, as "perhaps new;" and in a second despatch he said, "It is possible that this comet is the same as that discovered in America on the 2d of May." This conjecture, fortunately for Donati, did not prove true; although the apprehension of the Italian astronomer, from the rival zeal of his Transatlantic brethren, was not without reasonable foundation; for no sooner had the moon withdrawn from the evening sky, so as to allow the comet to be seen, than it was detected almost simultaneously at three different points in America, each observer being at the time unaware of its previous discovery in Italy. It was seen by Mr H. P. Tuttle on the evening of the 28th of June; and an accurate determination of its place was made on the same night at the Observatory of Harvard College. On the 29th, it was detected by H. M. Parkhurst, Esq., of Perth Amboy, N. J., and on the 1st of July, by Miss Mitchell, of Nantucket.

Three geocentric positions obtained on the 7th, 11th, and 13th of June, furnished DONATI with the means of computing approximate elements of the comet's motion, from which its interesting character was quickly recognised. Considerable difficulty was experienced in fixing the precise time of perihelion passage, a most necessary condition in predicting its path as seen from the earth. While in other respects the results deduced by various computors were sufficiently accordant, they showed wide discrepancies in designating the place of the comet in the orbit. By the middle of August, however, its future course, and great increase of brightness in September and the early part of October, had been ascertained with entire certainty.

Up to this time it had remained a faint object, not even discernible by the unassisted eye. It was distinguished from ordinary telescope comets only by the extreme slowness of its motion, in singular contrast with its subsequent career, and by the vivid light of the nucleus; the latter peculiarity was of itself prophetic of a splendid destiny.

Traces of a tail were noticed on the 20th of August, and on the 29th it was seen with the naked eye as a hazy star. For a few weeks it occupied a position in the heavens where it rose before the sun and set after it, becoming thus a conspicuous object both in the morning and evening sky. This circumstance give rise to the erroneous notion that two different comets had appeared. The statement which was widely circulated, that this was the return of the comet of 1264 and of 1556, supposed by some to be identical, is equally incorrect. If it has ever before been seen by man, it must have been far back in history, since the most recent computations assign a time of revolution of about twenty-five hundred years.

On the 6th of September was first noticed the curvature of the tail, which subsequently, at the time of its greatest expansion, became one of its most impressive features. It is remarkable that this peculiarity should have been strongly enough exhibited to be distinguished at the above date, when the earth was close to the plane of the comet's orbit. The observation cannot in fact be reconciled with the commonly received opinion that the curvature of the tail lies in the plane of motion about the sun.

The extraordinary changes exhibited in the neighbourhood of the nucleus during the disruption of the envelopes, and by the train while the comet was in the part of its orbit nearest the sun, will be illustrated in the conclusion of this article by a series of figures. This method of description is the only one by which an adequate conception of the condition of the comet, during this critical period in its history, can be conveyed to reader.

Part II.—*An account of the Comet of Donati.* 1858.

For the following details, relating to the appearance presented by the comet in the telescope or to the naked eye, use has been made principally of the manuscript records of the Observatory of Harvard College, the results of observation made elsewhere not being accessible through the ordinary channels of information at the time of writing.

There was a marked increase of brilliancy, accompanied by an equally perceptible lengthening of the tail, between the

10th and 25th of September. Its sudden advance in size and splendour during the week following the latter date, was in perfect keeping with the often-repeated history of bodies of its class. Every condition seemed to favour a rapid development. It was approaching the sun, which not only subjected it to a more intense illumination, but probably to other influences of a nature not well understood, but perfectly obvious in their effects; it was drawing nearer also to the earth, and was, besides, every night taking a more favourable position above the horizon, and presenting to better advantage the length and curvature of its train; lastly, the absence of moonlight towards the end of the month contributed more than any other circumstance to enhance the grandeur of the scene. Scarcely less impressive was the sudden vanishing of the spectacle a few weeks later, attributable to similar influences as it were reversed;—the comet receding from the sun and earth, contracting its dimensions, descending low in the mists of the horizon, and finally almost extinguished by the returning moonlight.

On the 8th, the diameter of nucleus was ascertained to be two thousand miles. In immediate contact with it, was an intensely brilliant nebulosity, having a diameter of about three thousand miles, while the surrounding diffused light extended forty or fifty thousand towards the sun. Measured by ordinary standards, this latter distance appears large, but it was manifestly insignificant compared with the effusion of nebulosity in the direction of the tail. Indeed the comparative absence of any considerable collection of diffuse light on the side nearest the sun, outside of the above radius, was so noticeable as to excite remark on several subsequent occasions. The fact of the position of the nucleus, precisely in the vertex of the train, must have been generally noticed. At this date the tail had acquired a length of sixteen millions of miles.

To ascertain the true dimensions of the nucleus, and to compare the intensity of its light with that of a star of equal brightness, the comet on the morning of the 9th was kept in the field of the great refractor by the clock-work motion, and the effect of the approach of daylight upon it carefully noted.

In the early twilight the nucleus resembled a star of the fifth magnitude, subtending an angle corresponding to a diameter of five thousand miles; but owing partly to atmospheric disturbances, and partly to the difficulty of distinguishing its precise border, this proved to be much too large, for it diminished to less than half that amount when the daylight had become sufficiently strong to obliterate all but the true centre, which continued in sight until twelve minutes before sunrise; the light however no longer retained the scintillating, star-like character which distinguished it when seen on the background of a dark sky. At this time, therefore, the nucleus must have been nearly of the size of our moon, and probably shone with somewhat inferior intrinsic brightness.

Sept. 12.—" A rapid increase in brightness, and length of train, the latter covers an arc of 6°."

" The intensity as well as the quantity of light emanating from the nucleus are the most distinctive features. To the naked eye, aided by the light of the envelope and contiguous part of the tail, it was as bright as a star of the third magnitude. In the telescope the light, concentrated within a circle of 10″ diameter, or six thousand miles, resembles that of a star of the fifth or sixth magnitude diffused over an equal space." The view of the comet on the morning of the 13th was still more satisfactory, owing to its greater elevation and the absence of moonlight.

" To the naked eye on the 17th, the head equalled a star of the second magnitude. Its southern side (on the left hand and below as seen in the evening) was decidedly the brightest. A similar contrast was noticeable through a considerable extent of the tail for several weeks following; the convex outline being both brighter and more clearly defined than the opposite side. Ultimately this distinction disappeared, or rather it was reversed; the change taking place gradually, and becoming most noticeable after the 6th of October.

" The commencement of a most important epoch in the physical history of the comet dates in our records from the 20th of September. It is probable that symptoms of approaching changes, faintly indicated, may have appeared somewhat earlier; they were not, however, noticed on the 17th and 18th, on both of which occasions the comet was observed, though particular attention was not then given to the condition of the nucleus.

"On the evening of the 20th the train at its origin was plainly bifurcated, issuing from the head in two unequal streams forming its two sides, and leaving between them a dark space behind the nucleus. Their outline was a curve resembling an arc of the parabola or hyperbola. The southern stream was so much the more brilliant of the two, that in strong twilight this alone would have been seen as a short tail inclined by 30° or more to the true axis. "Between the nucleus and the sun is interposed an obscure crescent-shaped outline, within which the light is unequally distributed, and has a strangely confused chaotic look; the details are too undecided for precise description. There is also an elongation of the nucleus, which is singularly brilliant, or perhaps a ray extending a few seconds from it on the following or upper side. The exact character of these phenomena could not be made out, but they seemed to indicate the presence of some internal disturbing force."

The obscure crescent-like band was so faintly marked that it might easily have been overlooked, or have passed for an illusion.

Sept. 23.—"A fine clear sky with the moon nearly full. To the naked eye the head of the comet is as bright as a star of the first magnitude, and the train, notwithstanding the moonlight, is 6° or 8° long. It is already a brilliant object half an hour after sunset. The telescopic view is most extraordinary. The nucleus has diminished in size, being now only 3″, or 1300 miles in diameter. Its light is exceedingly intense, and somewhat more concentrated than on the 20th. Outside of it is a bright envelope with its vertex in the direction of the sun, and 15″, or 6400 miles distant. This is bounded on its outer margin by a dark band. The boundary of a second and less brilliant envelope is distant at its vertex about 30″, or 12,800 miles from the nucleus, and is terminated by a similar dark arch, outside of which, again, is an atmosphere of faint diffused nebulosity rapidly shaded off. The outlines can be distinguished through an arc of 220° or more, reckoned from the nucleus, but they extend considerably further into the train on the following or bright side."

The form and situation of the envelopes and their relative degrees of brilliancy, are represented in fig. 3 by gradations in the character and strength of the lines in the engraving.* The scale to which this figure is drawn has not been ascertained from exact micrometer measurements, and is not to be implicitly relied on in comparing the dimensions of the envelopes at this date with others where the proper angles at several points were more carefully determined.

Sept. 24.—The train is 7° in length, and evidently curved. The telescopic view showed a decidedly dark axis to the tail, extending close up to the nucleus, which was elongated in a direction perpendicular to the axis. The inner envelope south of the nucleus was in part separated from it by a darker space, and was twice as bright as the one next outside of it, which in turn was much brighter than the exterior nebulosity.

The dimensions of the two envelopes on the 24th were found to be as follows:—

Width of inner, ... = 5800 miles,
Chord through the nucleus to the outer edge of the inner, ... = 17,000 „
Distance from nucleus to outer edge of outer, = 13,400 „
Chord through nucleus and the outer edges of the outer envelope, . . . = 31,000 „

The obscure bands on the margins of the envelopes were 3″·5 or 1400 miles broad. The estimated angle of divergence of the two forks of the tail at a distance of 10′ from the nucleus was from 20° to 25°.

On the 25th, the nucleus presented itself under a new aspect, in the act, as it afterwards proved, of disengaging a new envelope, or rather in a stage preparatory to that event. This perhaps is the first instance where an envelope has been seen in embryo at the surface of the nucleus, and has been traced through successive stages to a full development. The same phenomenon was subsequently illustrated in the case of the present comet by several exhibitions of a similar nature; their history has a peculiar value, because it affords an insight into mysterious processes by which the train is thrown out from the nucleus, under the stimulating influence of the sun's light and heat, or possibly of some unknown emanation from the same source. The following were the most conspicuous gradations of light recognised in its neighbourhood. Commencing with the dark axis we have:—

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* The parallel curved lines in the figure are employed merely to indicate the places where the light was more or less intense, and must not be too literally interpreted. The general effect is better given in fig. 1; but all attempts at a faithful representation by means of an engraving must, from the necessity of the case, be inadequate.

1*st.* The axis, a narrow, well-defined dark stripe penetrating quite up to the central body. Next in order towards the sun is the nucleus, on the eve, as we may say, of an eruption. The expression is fully warranted by its subsequent history. When seen to best advantage, two little streams of luminous matter were observed issuing from it one on each side, doubtless on their way to supply material to the tail now so rapidly expanding. Outside of the nucleus and of the nebulosity, apparently adhering to it, was a comparatively dark space, succeeded by two envelopes with the intervening dark bands, and lastly, over the whole a thin veil of diffuse light; the latter attaining a distance of seventy thousand miles. If we include the dark axis, and the dark background of the sky, we have here nine alternations of light and shade, of various grades of intensity.

The micrometric measurements furnish the following dimensions,—

From the nucleus to the outer edge of the inner envelope, ...	= 7,100 miles,
Length of chord from outer edge of inner envelopes through the nucleus, .	= 15,600 „
From the nucleus to the dark space outside of the nebulosity, ...	= 3,500 „

This space, first seen on the 24th, continued to enlarge until the first envelope was completely severed from connection with the nucleus. The breadth of the obscure band outside of the first envelope was 1600 miles. At a distance of about three hundred thousand miles from its origin, the breadth of the tail was found to be one hundred and forty thousand miles. Its extreme length was 11°, and the breadth where widest 1°.

Sept. 27.—We have now conclusive evidence that the condition of the central luminosity on the 24th and 25th was that of an envelope in its earliest stages. "The outline of a new envelope is clearly distinguished. In form and position it is a miniature of that which has hitherto been the innermost. Like the latter it sets awry, inclining to the right hand side of the axis. The outline of the second envelope is becoming indistinct. The narrow dark stripe in the axis having its vertex precisely at the nucleus is a remarkable object;" its width near its origin was found to be 1800 miles.

The tail has now attained a length of 13°, or eighteen millions of miles. A new appendage in the form of a long narrow ray issuing from its convex side is seen as represented in fig. 4, not following the curve of the tail proper, but projected nearly in a straight line from the sun. Its appearance simultaneously with the throwing off of a new envelope suggests the possibility of the two phenomena being in some way connected with each other; both, it will be noticed, lie on the same side of the axis. Supposing it to have started from the head of the comet on the 25th, its velocity must have reached eight or ten millions of miles daily. Other comets have exhibited similar rays. That of 1843 shot out its streamers to a much greater distance. One which appeared in 1744 is said to have had no less than six, spread out like a fan.

On the 28th, the image of the nucleus in the focus of the large refractor afforded distinct photographic action, but the surrounding luminosity was not intense enough to form a picture. "The dark opening in the axis of the tail occupies about one-twelfth of its breadth at a distance of 1° from the nucleus; it may be traced distinctly one or two degrees. The head of the comet, seen with a small telescope in strong twilight, which obliterates all but this brightest portion, is crescent shaped. The tail is 19° long, or twenty-six millions of miles, with a streamer as on the 27th."

Sept. 29.—Between this date and the 30th the comet passed its point of nearest approach to the sun, being distant about fifty millions of miles, and not quite seventy millions from the earth, which it was still rapidly approaching.

"Marked changes have occurred since the 27th. The little half moon envelope, then closely shrouding the nucleus, has elevated itself above it, and become the most conspicuous feature in the telescopic view. It is brightest near its outer edge," an indication that it was about to separate from the central nebulosity.

Fig. 5 will serve to convey an idea of the disposition of the envelopes; $a\ a'\ a''$ was fast losing its contour, which could with difficulty be made out; its place in the figure is not much to be trusted. It will be remembered that within five days all the nebulosity within the outline $c\ c'\ c''$ had been thrown off from the surface of the nucleus rising from it at the rate of a thousand miles daily. There is reason to suppose that the evolution was attended with something of violence, or of the nature of a sudden disruption, or of an explosion, if the expression does not convey too much the idea of motion apparent to the eye. There were rays or jets of light streaming in different directions from the centre, one, in particular, on the following (apparent right hand) side, imperfectly suggested on the 27th, now plainly seen, and there was a general aspect of confusion, suggesting the idea of internal disturbances. It was afterwards observed, that as the nebulous matter rose higher and higher above its origin, it became uniformly blended, as if, when relieved from the immediate neighbourhood of the nucleus, it was disposed to an even and symmetrical arrangement. The measured arcs gave the following results, n in fig. 5 designating as usual the nucleus, and b' and c' the vertices of the envelopes.

$n\ b' = 10{,}500$ miles.
$n\ c' = 6{,}000$ „

The thickness of the brightest part of the arch under b' was 2000 miles. Since the 24th, b' has ascended (towards the sun) about 5000 miles, or only about one three-thousandth part of the distance over which the end of the tail has advanced during the same interval.

Sept. 30.—" The edge of the envelope $c\ c'\ c''$ is very distinct, and may be traced through an angle of 270°, reckoned from the nucleus. The latter is truncated, as it has often before been seen on the side opposite to the sun, giving it a half-moon shape. The dark axis, which at its origin is almost black, and is of even breadth with the nucleus, completes the resemblance to a *phase* and *shadow*." There are objections to this explanation, although at first sight it is very plausible. Each new envelope, as it emerges from the nucleus, has the same phase-like form, while it is certainly everywhere permeated by the sunlight; a very small envelope still adhering to the nucleus would thus explain the peculiar form of the latter. The dark axis occupies a larger proportion of the whole breadth of the train at a distance of several degrees from the nucleus than can with any probability be attributed to the defect of light intercepted by so small a body. It is, moreover, curved, which could not happen to a sensible amount in the shadow.

Perhaps two phenomena are here superimposed; a comparative deficiency of nebulosity towards the central regions of the tail, and an actual shadow, perceptible a short distance only, close to the head of the comet, where, at any rate, we must assume the existence of a considerable collection of nebulous matter, sufficient to exhibit the outlines of a shadow cast upon it, if such really exists. This view receives some confirmation from a note of a later date. "The outlines of the axis-band are *straight lines* near the nucleus, but at a little distance they begin to blend with the general deficiency of light in the middle of the train." It seemed here to be conceivable that the shadow-margin and the outlines of the axis were distinct phenomena.

The tail to the naked eye was 22° long, or twenty-six millions of miles, and from 2° to 3° broad near its extremity, where also its rate of curvature was pretty suddenly increased. The upper outline was throughout brightest and best defined.

Oct. 2.—No new envelope had yet been formed, nor were any indications of its approach manifested, although they were carefully looked for in the expectation that one would shortly appear. The nucleus, however, was unusually bright, and rounded on the side toward the sun. An increase of brilliancy in the nucleus was afterwards recognised as the precursor of a fresh eruption from its surface. Its diameter, perpendicular to the axis, was found to be less than 1600 miles.

There were three dark openings in the innermost envelope, between which it was intersected with bright rays. In Plate I. (omitted), the engraver has given an eminently successful representation of the comet as it appeared in the field of the great refractor. The character of the light of the nebulosity composing the envelopes, and the appearance of the dark axial stripe penetrating, with well-defined outlines, quite up to the nucleus, have been preserved with great fidelity.

The long narrow ray first noticed on September 25, springing from the convex side of the tail, was seen again on the 2d of October,

The dimensions of the envelopes were as follows:—

$n\,b' = 13{,}200$ miles, $n\,c' = 7{,}500$ miles,
$b\,b'' = 3{,}300$ „ $c\,c'' = 18{,}900$ „

The breadth of the brightest part of the tail, at a distance of 144,000 miles from the nucleus, was 90,000 miles, and its extreme length from 25° to 30°.

The next date of observation was the 4th. Another envelope was then rising, having already attained a diameter of above nine thousand miles within forty-eight hours. Its peculiar form and the position of a dark spot, s, are given in fig. 6. A dark space now separated the envelope $c\,c'\,c''$ from the new one. In fig. 6, we have from the micrometer measurements,

$n\,c' = 8{,}900$ miles,
$n\,d' = 3{,}050$ „
$n\,s = 1{,}800$ „
$c\,c'' = 23{,}500$ „
$d\,d'' = 9{,}300$ „

The nucleus was smaller and less bright than on the 2d. The secondary tail was 35°, or thirty-four millions of miles long. On the 5th of October, the comet attained its greatest brilliancy. Its head was close to Arcturus, a star of the first magnitude, to which it was but little inferior in brightness, although the contrast in the *intensity* of their light was very evident. In Europe the two must have been seen still nearer to each other than they were in America, the nucleus passing a little to the south of the star, and the brightest part of the tail over it. The extremity of the train reached over Benetnasch and Mizar, the two southernmost stars in the tail of the Great Bear. It could be traced through an arc of 35°. Its breadth was 5° or 6°. With a little attention two additional streamers could be seen, one of which was between 50° and 60° long, or above fifty millions of miles, with a slight curvature, as in fig. 7.

The interest of the telescopic view, taking all the circumstances into account, the size of the instrument, the perfect purity of the atmosphere, and the splendour of the object, have rarely been surpassed. The nucleus and the outline of its nearest envelope were visible in full sunshine with the large telescope. The head of the comet could be seen with the naked eye at twenty minutes after sunset, at which time the second envelope was discernible with the telescope. It is most remarkable that, with all this accession of brightness, the nucleus itself had now diminished to a diameter of only four or five hundred miles, scarcely one-fifth of what it was on the morning of the 9th of September, by a very careful determination. Its volume had thus diminished to the *one-twentieth* part only. The remaining nineteen-twentieths had, in the intervening period, expanded into the tail, or had gone to form the envelopes which now encircled it, by a process which has been fully illustrated in the preceding pages. But are we then to conclude that the nucleus, the focus of these mysterious operations, had in this way expended the greater part of its substance? To this inquiry the best reply is a consideration of its subsequent condition. After several more eruptions from its surface, similar to those above described, it receded from our view about the 20th of October, with an evident *increase* of size compared with its condition two weeks before, and still shining with its accustomed intensity.

Examined in the daytime on the 5th with the highest powers which it would bear, no indication of a *phase* could be seen. The dark spot at *s*, of fig. 6, had expanded in about the same proportion with the whole envelope in which it was situated. From near the vertex, and from the sides of the latter, there seemed to be an escape of jets of luminous gas, which streamed off like light spray thrown up against an opposing wind and driven before it.

The dimensions of the envelopes were as follows:—

$n\,d' = 4{,}200$ miles, $n\,c' = 9{,}500$ miles,
$d\,d'' = 8{,}900$ „ $c\,c'' = 27{,}000$ „
$n\,b' = 14{,}200$ miles,
$b''\,b = 40{,}500$ „

The diameter $d\,d''$ passed considerably above the nucleus. The outline $a\,a'\,a''$ of previous figures has faded away, and it is uncertain whether $b\,b'\,b''$ is the margin of the envelope so designated, or only that of the pretty sudden terminus of the stronger light comprising $a\,a'\,a''$ as well. The extreme diffusion of the light at some of the points measured occasions a good deal of uncertainty in the above numbers; the distances to the vertices are usually the most trust-worthy.

It will not be necessary to enter into the details of the history of other envelopes further than to indicate some of their leading features. Between the 2d and the 20th of October inclusive, four of them rose in succession from the nucleus. One, which was first seen on the 4th as just described, one between the 8th and 9th, another on the 15th, and a fourth on the 20th. The outlines of the brighter parts of three of them are shown in figs. 8, 9, 10, 11.

Fig. 8 is the outline, on October 6, of the envelope which made its appearance between the 2d and 4th; the places are indicated where it was intersected by brighter rays: its interior structure was very irregular. In fig. 9, we have a representation of the inner envelope on October 11, the brighter portions only being included. Fig. 10 gives the outlines of the envelope first seen on October 15. Fig. 18 shows it at a more advanced stage, three days later.

A change in the relative proportion of light distributed on the two sides of the principal axis of the comet had been progressing up to about the 6th of October. At this date, although there may still have been a little more light on the right hand side, the difference was not nearly so large as it had been. The diameter of the nucleus was then 800 miles. On the 8th, its diameter was 1100 miles. The envelopes were most distinct on the left hand, or preceding side. The change was a permanent one, and for the future this became the brightest half of the head of the comet. It is curious to observe a corresponding change in the inclination of the envelopes to the axis. They now inclined even more decidedly to the left hand than they had at first done to the opposite side. The two last, those of October 15 and 20, seem in fact, in the first instance, to have issued as luminous jets or streams from the side, rather than from the vertex of the nucleus. A similar reversal in the order of brightness was evident in the part of the train near the nucleus.

The tenth was the day of nearest approach to the earth, but the comet was manifestly on the wane, though expanded over a larger extent of the sky than before. Five envelopes, reckoning the exterior haze as one, could be traced through the whole or some part of their outline. The dark stripe of the axis was becoming less conspicuous, the central regions of the train being occupied with diffused light; on the 11th it was barely discernible. The last of the envelopes was thrown off on the 20th. The comet had now passed far to the south, and its low altitude prevented the continuance of the observations.

We must add a few words on the appearance presented by the tail between the 6th and the 10th of October. At the date first named, one of the supplementary rays attained a distance of 55°, or fifty millions of miles from the nucleus, somewhat exceeding that of the principal tail, and in a direction, as usual, nearly in a line from the sun. Others less perfectly developed could be decerned near a point where the curvature of the main stream was pretty suddenly changed. On the 8th, fig. 12, five or six transverse bands could be distinguished in the tail half a degree or less in breadth, with clear, well-defined outlines, and perfectly resembling auroral streamers, excepting that they kept their position permanently, that is, without motion sensible to the eye, they diverged from a point between the sun and the nucleus. The supplementary ray was not inserted in the original drawing from which fig. 12 was engraved. Its place in the figure has been supplied from sketches on the 9th, and dates previous, allowing for its motion in the interval.

The train attained its largest apparent dimensions on the 10th, when the main stream of light could be distinguished through an arc of 60°, corresponding to a length of fifty-one millions of miles, or rather more than half the distance of our earth from the sun. The distribution of its light at a distance of 20° or 30° from the nucleus in parallel or slightly diverging bands, alternating with dark spaces, was strongly exhibited. Thoy were 5° long, and 20′ or 30′ wide, and might aptly be compared either to the streamers which often break up the continuity of an auroral arch, or to a collection of five or six tails of small comets forming from the remains of the large one. Whatever may have been their real nature, the impression to the eye involuntarily suggested the comparison. These bands were visible for one or two succeeding evenings, but were soon overpowered by the moonlight.

We will conclude with a review of some particulars relating to the comet which seem to deserve special attention. The dimensions of the tail, and of the nucleus and envelopes on the several dates of observation, are given below. Apparent variations in the size of the nucleus were sometimes caused by disturbances in our own atmosphere, but in most cases the changes were undoubtedly real ones. The presence of moonlight, or of the slightest haze in the sky, had a very perceptible effect in diminishing the arc through which the tail could be traced. This will sufficiently explain the irregularities noticed in comparing its proportions from night to night.

Date.	Length of tail.	Breadth at extremity.	Remarks.
1858.	Miles.	Miles.	
Aug. 29.	2°= 14,000,000		
Sept. 8 & 9.	4 = 16,000,000		
„ 12.	6 = 19,000,000		
„ 17.	4 = 10,000,000		Moonlight.
„ 23.	7 = 12,000,000		„
„ 24.	7 = 12,000,000		„
„ 25.	11 = 17,000,000	1,500,000	
„ 27.	13 = 18,000,000		
„ 28.	19 = 26,000,000		
„ 30.	22 = 26,000,000	3,000,000	
Oct. 2.	25 = 27,000,000	5,000,000	
„ 5.	35 = 33,000,000	5,000,000	
„ 6.	50 = 45,000,000		
„ 8.	50 = 43,000,000	7,000,000	
Oct. 10.	60 = 51,000,000	10,000,000	

Date.		Length of tail.	Breadth at extremity.	Remarks.
1858.		Miles.	Miles.	
,,	12.	45 = 39,000,000		
,,	15.	15 = 14,000,000		Moonlight.

Date.		Length of "Streamers."	Breadth at extremity.
1858.		Miles.	Miles.
Oct.	4.	35°= 34,000,000	1,000,000
,,	5.	55 = 53,000,000	,,
,,	6.	55 = 50,000,000	,,

In computing the above, the curvature has not been regarded. It must be borne in mind that we have taken for the extremity of the tail the furthest point at which it was possible to detect a trace of it. It would scarcely have been noticed beyond 30° or 35°, even between the 5th and 10th of October, without a particular effort of the attention, and some training of the eye. The streamers, or additional rays, might easily have escaped notice altogether from their faintness. In making a comparison of the size of this comet with others, it will be best to limit the extent of the tail to the arc over which it was plainly visible, which would give a length of about thirty-five millions of miles. The shortening of the tail between the 12th and the 17th of September is due entirely to the effect of moonlight. The more abrupt change between the 10th and 15th of October is partly due to the same cause, but there must also have been a great diminution in brilliancy.

For the nucleus we have the following measured diameters:—

1858, *July* 19.—Diameter 5″=5600 miles. This probably includes the dense nebulosity immediately surrounding it, not distinguishable at the time from the true centre, on account of the low altitude of the comet.

Aug. 19.—"Nucleus equals a star of the seventh magnitude."

,, 29.—Head of the comet visible to the unassisted eye as a star of the sixth magnitude.

Aug. 30.—Diameter 6″=4660 miles. This result perhaps includes more than the true nucleus.

Sept. 8, 9.—Diameter 3″=1980 miles. Taken just before sunrise, when all of the comet, excepting the nebulosity next the outside, which was 3300 miles in diameter, was obliterated. On a dark sky the apparent diameter was 5280 miles, and the light equivalent to that of a star of the fifth magnitude.

Sept. 12.—To the naked eye the head of the comet appeared as a star of the third magnitude.

On *Sept.* 17, it equalled a star of the first magnitude.

Sept. 23.—"To the naked eye the head of the comet is brighter than a star of the first magnitude." Its brilliancy at this date (one week before its perihelion passage, and seventeen days before its nearest approach to the earth) had reached a maximum. It is interesting to remark, that between the 17th and 23d was first noticed the characteristic formation of envelopes, which plainly operated as a check upon the accumulation of brightness at the central point. The nucleus, during the remaining period of its visibility, went through a series of periodic changes, acquiring more light just before an eruption, and suddenly diminishing after it. The variations, although evident to the eye, could not be accurately measured on account of the smallness of the angle subtended, and its want of precise definition.

On the 23d, its diameter, which appeared to be less than usual, was 3″=1280 miles.

Sept. 24.—Diameter 2″·5=1030 miles.

Oct. 2.—Diameter 5″·2=1560 „

„ 4.—Nucleus evidently smaller than on the 2d.

„ 5.—Diameter 1″·5=400 miles; "it is certainly less than 2″=540 miles." This determination was made under most favourble conditions.

Oct. 6.—Diameter 3″=800 miles. The head of the comet nearly equalled Arcturus.

Oct. 8.—Diameter 4″·4=1120 miles. "The nucleus is decidedly brighter than on the 6th, and is preparing to throw off a new envelope."

Oct. 9.—The nucleus had diminished in size simultaneously with the appearance of a new envelope.

Oct. 10.—Diameter 2″·5=630 miles.

„ 11.—Diameter 2″ =510 „

„ 15.—The head of the comet was as bright to the naked eye as a star of the third magnitude.

Oct. 18.—Diameter 3″= 900 miles.

„ 19.—Diameter 3″= 920 miles. The nucleus was compared with three stars of the sixth magnitude at the same altitude, and found to be far brighter than either of them. It was probably at least as bright as a star of the fifth magnitude, while to the naked eye the head nearly equalled one of the third magnitude.

Oct. 20.—Diameter 2″ = 660 miles. A new envelope was forming.

As before remarked, the least observed diameter of the nucleus, 400 miles, occurred on October 5th, the evening when the comet reached its maximum of brightness.

In order to exhibit the progressive motion of the envelopes from their point of origin, we give below in one view the distances of their vertices from the nucleus at different dates. The distances were measured in the line from the nucleus towards the sun. The better to distinguish them, we will use the following notation, which is the same with that employed in the figures.

a' = Vertex of envelope first seen on Sept. 20.
b' = „ „ „ „ 23.
c' = „ „ „ „ 27.
d' = „ „ „ Oct. 4.
e' = „ „ „ „ 9.
f' = „ „ „ „ 15.
g' = „ „ „ „ 20.

Distances in miles from the nucleus (n) to the vertices of the envelopes *a*, *b*, and *c*.

	na'	nb'	nc'	nd'	ne'	nf'
Sept. 23.	*13,000	*6,400				
„ 24.	13,400	5,800				
„ 25.	*18,000	7,100				
„ 27.		8,400	3,500			
„ 29.		10,500	6,000			
Oct. 2.		13,200	7,500			
„ 4.			8,900	3,050		
„ 5.		14,200	9,550	4,210		
„ 6.			10,100	4,270		
„ 8.			12,400	7,160		
„ 9.			13,200	8,650	1,910	
„ 10.			14,100	8,780	2,760	
„ 11.				*10,200	*4,200	
„ 15.				*11,400	8,160	3,200
„ 18.				*14,500	9,950	4,400
19.					11,200	5,500

* The numbers marked with an asterisk are less reliable than the others.

The vertex g' had barely left the surface of the nucleus on the 20th.

The comet of Donati, although surpassed by many others in size, has not often been equalled in the intensity of the light of the nucleus. The diameter of the surrounding nebulosity on the other hand was unusually small, never much exceeding one hundred thousand miles, while that of the great comet of 1811 was ten times larger—its envelope attaining an elevation of more than three hundred thousand miles above the central body, exceeding by more than twenty times the largest of our measurements given above. Still it would be difficult to instance any one of its predecessors which has combined so many attractive features.

Its early discovery enabled astronomers, while it was yet scarcely distinguishable even with the telescope, to predict, some months in advance, the more prominent particulars of its approaching apparition, which was thus observed with all the advantage of previous preparation and anticipation. The perihelion passage occurred at a most favourable moment for presenting the comet to good advantage. When nearest the earth, the direction of the tail was nearly perpendicular to the line of vision, so that its proportions were seen without foreshortening. Its situation in the latter part of its course afforded also a fair sight of the curvature of the train, which seems to have been exhibited with unusual distinctness, contributing greatly to the impressive effect of a full-length view. Frequent allusion has been made to the influence of the light of the moon on the visibility of the comet. Few readers will be aware how much of its splendour and vast dimensions, during the first ten days of October, we owe to the fortunate circumstance that, at this critical period, the moon was absent from our evening skies. The effect of the presence of a full moon, though simply optical, and due only to the force of contrast, would have been quite as prejudicial as if the comet had lost two-thirds of its train, and as large a proportion of the brightness of the remaining third; above all, we must have lost those most singular phenomena—the supplementary rays, and the alternating bright and dark bands in the train; the latter seem to have been new in cometary history. Supposing the substance of the tail to be driven off into space, never again to return to its original source, the inquiry at once arises, What then becomes of it? The appearance in question shows plainly enough a process of separation into distinct masses, and in each of these a tendency to condense about a central axis.

It is remarkable that the aggregation should have been around separate axes, rather than about one or more central points, and that the axes should have manifested a disposition to diverge from the sun; as though these collections of nebulosity were in reality a group of new comets in process of formation. The increasing moonlight and low altitude of the comet would not allow their being followed to a more complete development.

The condition of the nucleus and neighbouring region has received a large share of attention in the preceding pages, because it has afforded so ample an illustration of phenomena of which, up to the present time, very little has been certainly known. The comets of 1744 and of 1811 had well-formed envelopes, but the observations upon them were too imperfect and disconnected to afford much more than a basis for conjecture as to their origin and destination. That of Halley, at its apparition in 1835–36, furnishes an example more nearly parallel to the present one, but its phenomena were on a comparatively feeble scale.

The most recent intelligence leaves no room to doubt that the comet of Donati is periodical, having a time of revolution of about two thousand years. The following are the results arrived at by different computers:—

WATSON,	2415	years.
BRUHNS,	2102	"
LÖWY,	2495	"
GRAHAM,	1620	"
BRÜNNOW,	2470	"
NEWCOMB,	1854	"

The last two determinations are based upon longer intervals of observation than the others, Mr Newcomb's being a few days longer than that of Dr Brünnow. The remaining uncertainty in the period will be materially reduced, when observations have been received from the southern hemisphere, where the comet is still in sight.

The subjoined table contains the distances of the comet from the sun, and from the earth, and its hourly rate of motion:—

1858.	Distance from Sun in miles.	Distance from Earth in miles.	Hourly Velocity in miles,
June 2,	215,000,000	240,000,000	65,000
July 2,	173,000,000	240,000,000	72,000
Aug. 2,	127,000,000	220,000,000	84,000
Sept. 1,	82,000,000	160,000,000	105,000
" 11,	70,000,000	130,000,000	115,000
" 21,	60,000,000	95,000,000	124,000
Oct. 1,	56,000,000	66,000,000	128,000
" 11,	61,000,000	52,000,000	123,000
" 21,	71,000,000	67,000,000	114,000

Supposing its last perihelion passage to have occurred at the beginning of the Christian era, it must have passed its aphelion in the early part of the tenth century, at a distance of 14,300 millions of miles from the sun, its velocity at that point being 480 miles an hour.

Description and Analysis of three New Minerals, Associates in the Trap of the Bay of Fundy. By Henry How, Professor of Chemistry and Natural History, King's College, Windsor, Nova Scotia.

The minerals of which I propose giving an account in the present paper, were found in June 1858, by Dr Webster, of Kentville, N. S., and myself, in the trap rock of the Bay of Fundy, on the shore of Annapolis county, N. S., a couple of miles or so east of a headland called Black Rock, which is well known to the navigators of the bay, from its being of considerable height, and jutting some distance into the water.

On this occasion, in our search for specimens, we landed about a mile east of Black Rock and walked eastwards, which circumstance I mention because it afforded us an opportunity of observing the nature of the rocks under which we travelled.

As is well known to those who have visited these shores, the Bay of Fundy presents, on the Nova Scotia side, three varieties of trap, viz., the tufaceous, the vesicular amygdaloidal, and the more compact crystalline rock. The cliffs under which we passed for some distance had the last named character, being dark blue-black in colour, as indicated in the name Black Rock, frequently perpendicular, and showing little evidence of the recent action of water and frost, from the absence of any fragments but such as were small, worn smooth, and rounded by long attrition on the beach. I do not remember having observed any columnar structure. We experienced the known barrenness of this kind of rock in specimens, having obtained comparatively few in a toilsome journey; the species observed were principally apophyllite and laumonite; among those we found, however, were the subjects of the investigation now published.

These minerals composed the mass of a solid reniform nodule, about half the size of a fist, partly imbedded in the crystalline trap adverted to above. The nodule was covered over the greater part of its surface with a dark green coating, spangled with crystals, apparently of chlorite; the portion free from this coating showed irregularly hemispherical protuberances of a yellowish colour, and stellated appearance. The nodule yielded with difficulty to the hammer, and when broken presented a curious internal structure.

Immediately beneath the thin green coating was observed a narrow band of yellowish white mineral resembling wax, isolated patches of which, few in number, occurred a little removed from the rind, among spherical concretions having a most distinct stellated appearance, and, in portions, a highly pearly lustre, while the centre was principally made up of a bluish-gray opaque-looking mineral in rounded spots. The components of the nodule were so closely packed that only one very small cavity was observed, the margins of which had a radiated structure.

The only difficulty experienced in determining the nature of this mass was in the separation of its constituents, which, from their very intimate association, proved an extremely tedious process; while, from their containing the same elements in different proportions, great care was requisite to ensure trust-worthy results on analysis.

Cyanolite.—The mineral mentioned as most abundant in the centre of the nodule was found to present no crystalline structure. Its hardness = 4·5, very coherent in the mass, rather brittle than tough in small pieces; S.G. = 2·495 in coarse fragments, fracture flat-conchoidal, even; streak white; lustre dull, colour bluish-gray; subtranslucent in very thin pieces, translucent on the edges, powder transparent under the microscope. A fragment not altered in strong nitric acid, its powder did not gelatinize with hydrochloric acid before or after ignition, but afforded slimy silica; in matrass became white, giving off water; before the blowpipe in platinum forceps, the edges only of thin splinters were rounded in a good heat; with borax and with soda gave transparent beads, with salt of phosphorus a translucent glass.

The results of the following analyses were obtained by first igniting the powdered mineral for water, and then treating the residue with strong hydrochloric acid, digesting and evaporating to dryness. The resulting silica, after being dried and weighed, was fused with carbonate of soda, the fused mass, being treated in the usual way, was found to yield very nearly the amount (under 0·8 per cent. less) of silica first obtained; the second weight was taken as the correct estimate, and the small quantities of alumina and lime now set free were determined, and added to those from the original acid fluid. This method, though rather tedious, served to economise a material not readily obtained in a state fit for analysis, one quantity furnishing a knowledge of all the constituents.

The results, afforded by the mineral immediately after being powdered, were—

	I.		Oxygen.	Ratio.
Lime, . .	17·52	=	5·00	1·
Alumina, . .	0·84	=	0·39	
Magnesia, . .	trace.			
Potass, . .	0·53	=	0·09	
Silica, . .	74·15	=	39·28	7·85
Water, . .	7·39	=	6·56	1·31
	100·43			

Another experiment, upon what I consider to be this mineral less perfectly freed from that which will be next described, afforded—

	II.
Lime, . . .	18·19
Alumina, . . .	1·24
Magnesia, . . .	trace.
Potass, . . .	0·61
Silica, . . .	72·52
Water, . . .	6·91
	99·47

which numbers agree sufficiently well with the first, under the circumstances, to show definite composition, the formula expressive of which, however, I deduce from the first analysis. The alumina and potass are evidently not essential constituents, and probably* replace a portion of the lime, and the ratio of oxygen in the lime, silica, and water, as found, is 1 : 7·85 : 1·31; taking this as 4 : 31·40 : 5·2, I propose as the formula of cyanolite

$$4\ CaO,\ 10\ SiO_3 + 5\ HO,$$

which requires the per centage,

$4CaO$	$=112$	$-18·36$
$10SiO_3$	$=453$	$-74·26$
$5HO$	$=\ 45$	$-\ 7·37$
	610	100·00

according satisfactorily with the results of experiment.

It is perhaps worth observation that, if the water be taken as basic, the ratio of oxygen in all the bases to that of silica is 1 : 3.2, approximating to that in the anhydrous silicate of lime, Edelforsite, $CaO,\ SiO_3$, in which it is 1 : 3.

I have named the mineral Cyanolite (from κυανος cœruleus), in allusion to the blue tint which distinguishes it from its associates.

Centrallassite.—The association of this mineral with the preceding is very intimate; as already mentioned, cyanolite was found most abundantly in the centre of the mass in patches of a rounded outline, between which sometimes a transparent, sometimes an opaque white substance was seen; the latter being, as hereafter shown, a condition of the former, both presenting a stellated appearance; towards the exterior of the nodule this character was very decided, so much so indeed that I at first considered the mineral to be gurolite, which it resembles in some other points, and approaches in composition.

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* Dana, 4th ed., i., p. 208.

Centrallasite lies next the rind mentioned in a previous page, and to be more fully described presently, and where free from this shows elevations of a somewhat semi-globular external form, radiated on the surface; when broken, these are found to possess a lamellar structure, and to consist of plates diverging from a centre, forming truly spherical concretions; the surfaces of these plates have a highly pearly lustre, but the mineral passes into an opaque white condition, by a change which appears to commence uniformly at the centre, or to proceed from point of contact with cyanolite; this state is seen in fresh fractures of the interior of the nodule, and will be shown immediately not to arise from efflorescence. The name chosen for the mineral (from ϰεντϱον, centrum, and ἀλλάσσω, muto) is in allusion to this character. Centrallassite is white, sometimes yellowish, translucent, perfectly transparent in thin plates, which are easily obtained and readily broken *across*. It is rather brittle under the pestle, and is reduced to powder without difficulty; its lustre, sub-resinous, highly pearly on cleavage planes; hardness = 3·5; SG = 2.45 — 2·46. In matrass yields water, becoming opaque and silvery white, without exfoliation. Alone, before the blowpipe, fuses readily, with continued spirting, to an opaque glassy bead; with soda, dissolves in considerable quantity to a transparent glass; with borax, affords a transparent bead; with salt of phosphorus, dissolves slowly and entirely to a clear bead. A piece in strong nitric acid splits into translucent laminæ; in powder readily acted on by hydrochloric acid without gelatinising; after ignition affords flocculent silica on long digestion with the same acid.

Analysis afforded the following results. In No. I., separate quantities were taken for water and for the other constituents in II., the mode described in the case of cyanolite was adopted, and was found to reduce the amount of silica obtained by digestion of the ignited mineral in acid by 1·5 per cent.; the last portions of water were not easily expelled; experiment was made on air-dry material, viz., immediately after its being reduced to powder:—

	I.	II.	Mean.		Oxygen.	Ratio.
Lime,	27·86	27·97	27·91	=	7·97	1.
Alumina,	1·00	1·28	1·14	=	0·53	
Magnesia,	0·20	0·13	0·16	=		
Potass,	undet.	0·59	0·59	=	0·10	
Silica,	59·05	58·67	58·86	=	31·18	3·91
Water,	4·40	11·43	11·41	=	10·14	1·27
	99·51	190·07	100·07			

I quote separately the results of another analysis, in which the portion of the mineral ignited for water was employed for the determination of the other constituents, and the silica not, as before, fused with carbonate of soda; the numbers obtained were—

	III.
Lime,	27·09
Alumina,	0·40
Silica,	61·10
Water,	11·03
Potass,	undet.
	99·62

and they are of value as showing that the amount of water is constant; and assuming, as may fairly be done, the same quantity of silica, &c., to be rendered insoluble, that the other constituents have, the relations observed in the preceding analysis upon the results of which alone I base my conclusions regarding the formula. The oxygen ratio of CaO, SiO_3, HO, evidently the essential components, is shown to be 1 : 3·91 : 1·27; taking this as 4 : 15·64 : 5·08 I deduce as the formula of centrallassite:—

$$4CaO, 5SiO_3 + 5HO$$

the per centages corresponding to which are,—

$$\begin{array}{lcrcr} \mathrm{4CaO} & = & 112\cdot & - & 29\cdot20 \\ \mathrm{5SiO_3} & = & 226.5 & - & 59\cdot06 \\ \mathrm{5HO} & = & 45\cdot & - & 11.74 \\ \hline & & 383\cdot5 & & 100\cdot00 \end{array}$$

and with these the experimental numbers agree very well, upon the view that a small proportion of lime is replaced by alumina, magnesia, and potass.

The formulæ adopted for the two minerals now described exhibit a very simple chemical relation, thus—

$$\begin{array}{ll} \text{Cyanolite} & = \mathrm{4CaO,\ 10SiO_3 + 5HO.} \\ \text{Centrallassite} & = \mathrm{4CaO,\ \ 5SiO_3 + 5HO.} \end{array}$$

they differ by five equivalents of silica. If, however, a part of the water in the analysis of the latter mineral be taken as basis, a correspondence may be traced between it and okenite; thus the formula being written,

$$\begin{array}{rl} \text{Centrallassite} & = \mathrm{(4CaO\ 3\tfrac{1}{2},\ HO),\ 5SiO_3 + 1\tfrac{1}{2}\ aq.} \\ & = \mathrm{4CaO, 5SiO_3 + 5HO,} \end{array}$$

may be compared with that of okenite, so written as to bring out the augitoid oxygen ratio* between bases and silica of 1 : 2, thus—

$$\text{Okenite} = \mathrm{3(CaO,\ HO),\ 4SiO_3 + 3\ aq. = 3CaO,\ 4SiO_3 + 6HO.}$$

and in both we have the ratio of oxygen in bases and silica as 1 : 2, with some additional water of hydration. And further, while the chemical composition of centrallassite is nearer that of okenite than of gurolite, its mineralogical characters resemble those of the latter very closely in some respects,† and the lime and silica in its adopted formula are those of these two minerals together, thus—

$$\begin{array}{ll} \text{Okenite} & = \mathrm{3CaO,\ 4SiO_3 + 6HO.} \\ \text{Gurolite} & = \mathrm{CaO,\ SiO_3 + 1\tfrac{1}{2}HO = (CaO\ \ SiO_3) + 3HO.} \\ \quad\text{Anderson.} & \\ \text{Centrallassite} & = \mathrm{4CaO,\ SiO_3 + 5HO.} \end{array}$$

The opaque condition of the mineral just described does not depend on loss of water; as already mentioned the concretions f pearly plates are often observed to be chalk-white towards the centre, and even in freshly exposed surfaces of the interior of the nodule spherical masses were seen, without lustre, chalk-white, and in which a radiated structure was sometimes, but not invariably, quite obvious; on igniting a selection of such fragments as appeared most characteristic, the per-centages of water in two cases were found to be—

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* See "Dana's Mineralogy," 4th edit., pp. 301, 306, and Sixth Supplement to same in "Silliman's Journal," Nov. 1858, p. 363.

† Anderson, "Phil. Mag.," Feb. 1851.

	I.	II.
Water =	12·29	12·25

and the silica in the ignited residue of I. was found, by simple action of hydrochloric acid to amount to 61·11 per cent., or the same quantity as obtained from the transparent mineral (Analysis III., *ante*) under similar circumstances; these numbers satisfy me as to the identity of the minerals. The amount of water now exhibited is conclusive evidence that efflorescence had not occurred; and although the slight excess over the previous per-centages may suggest the idea that hydration was in progress, it is difficult to imagine such a process taking place in the interior of the consolidated mass, where the opaque condition appeared most perfectly developed, a situation in which the only obvious means of such change lies in the transference of water from cyanolite. It seems to me rather probable that water is more quickly absorbed by the opaque, somewhat powdery form, on exposure. In speaking of *change* occurring, I assume that the transparent lamellar form is the more perfect condition of the mineral, and although it is possible that the opaque may also have been an original form of deposit, I am disposed to look upon the latter as really resulting from the former, possibly in consequence of some molecular action. I must add, that one fracture of the nodule brought to view two very small tufts of divergent silky transparent needles, which showed, when magnified, a prismatic form, and which had the blowpipe characters of the pearly laminæ, whence I conclude that they consisted of centrallassite.

Cerinite.—The narrow band described as enveloping the two preceding minerals was about one-eighth of an inch in thickness; and small rounded concretions, having the same characters, were observed between masses of centrallassite (which was invariably next the band), and in these a concentric structure in layers was seen. The mineral is opaque, or subtranslucent in very thin fragments; is amorphous, its powder exhibiting under the microscope transparent grains without crystalline form; its lustre sub-resinous, it looks very like white, or yellowish white wax; hardness = 3·5; readily fusible, without intumescence before the blowpipe.

In the quantity selected for the first of the two following analyses, a few very minute red spots were visible; it was scarcely possible to free it *absolutely* from its associates. The first analysis having shown that the ignited mineral was very imperfectly attacked by acid, and that fusion with carbonate of soda was necessary for its decomposition, this method was at once resorted to in the second case. The results afforded by the mineral without artificial desiccation, were—

	I.	II.	Mean.	Oxygen.	Ratio.
Lime, . .	9·49	10·15	9·82	= 2·80	1·
Magnesia, .	1·83	1·91	1·87	= 0·76	
Potass, . .	*0·37	undet.	0·37		
Alumina, .	12·21	13·11	12·65	= 5·90	1·75
Peroxide of iron,	*1·01	1·27	1·14	= 0·34	
Silica, . .	58·13	57·02	57·57	= 30·50	8·56
Water, . .	15·96	15·42	15·69	= 13·94	3·91
	99·00	98·88	99·11		

the loss in these analyses probably proceeds from alkali not determined, and the ratio of oxygen between RO, $R_2\,O_3$, SiO_3, HO, as exhibited, may be taken as 1 : 2 : 9 : 4, which we have in the formula,

$$3CaO\,SiO_3,\ 2(Al_2\,O_3,\ 3SiO_3) + 12HO,$$

the per-centages from which,

3CaO	=	84	11·96
$2Al_2\,O_3$	=	102·52	14·60
$9SiO_3$	=	407·70	58·06
12HO	=	108·00	15·38
		702·22	100·00

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* As dissolved out by hydrochloric acid.

correspond very well with the experimental numbers for SiO_3 and HO, and are tolerably close to the aggregate amount of isomorphous constituents respectively.

No mineral of the same characters and composition has been, so far as I am aware, described; and, considering the associations, I look upon this as a new combination, and have named the mineral (from *κηρίνος*, cereus) cerinite, from its wax-like appearance. An examination of the formula just given shows it to contain the elements of edelforsite with those of two equivalents of stilbite.

$$\begin{aligned}\text{Edelforsite} &= \text{CaO SiO}_3\\ \text{2 Stilbite} &= \underline{2(\text{CaO SiO}_3, + \text{Al}_2\ \text{O}_3\ \text{Sio}_3 + 6\text{HO})}\\ \text{Cerinite} &= 3\text{CaO SiO}_3, + 2(\text{Al}_2\ \text{O}_3\ 3\text{SiO}_3 + 12\text{HO}\end{aligned}$$

The group of minerals now described appears to have resulted from deposition in this order, the cerinite being laid as a lining to the cavity of the trap rock, the centralassite began to be formed, and then the aluminous material was virtually exhausted in the small patches of cerinite interspersed among the accruing deposit, while the centrallasite and cyanolite seem to have been formed in alternating actions.

As respects chemical composition, the two latter minerals are interesting additions to the known hydrated silicates of lime, of which two only have been hitherto described, viz., okenite and gurolite; and they have a claim to our attention as ascertained products of the chemistry of inorganic nature. This is, to my mind, true of all results of chemical investigation into natural operations, whether they prove, on complete study, to refer to intermediate or to final stages of chemical action, as in the first case contributing to our knowledge of the course of changes, and in the latter exhibiting their perfection. These relations of the minerals now spoken of are seen on comparison of their formulas, as before given, from which view it is obvious that both centrallasite and cyanolite have more silica in their composition than the other described hydrated silicates of lime, and if we glance at the formulas of the two anhydrous silicates of the same base, along with that of cyanolite—

Wollastonite = $3CaO\ 2SiO_3$
Edelforsite = $CaO\ SiO_3$
Cyanolite = $4CaO\ 10\ SiO_3 + 5HO$,

we see that the last named is by far the most highly silicated combinations of lime yet met with. This new relation, if so decided a character between lime and silica, familiar to us as partial components of mineral substances in various approximative proportions, appears to me to render cyanolite especially interesting to mineralogists.

Great Eruption of the Volcano Mauna Loa, in the Island of Hawaii. Communicated by J. BARNARD DAVIS, Esq.

Mr W. L. Green has forwarded some papers respecting this occurrence, from which we derive the following particulars. On Sunday, the 23d of January 1859, the volcano broke out. The lava ran in a N.N.W. direction, and by the end of the month reached the sea at a place called Palaoa, nearly forty miles from the crater, where it destroyed a small fishing village.

The schooner "Kamoi" sailed from Honolulu with a number of persons on board to visit the neighbourhood of the eruption; and, on her return, an extra edition of "The Pacific Commercial Advertiser" was published, with a map of Hawii, describing the course of the lava-stream. Copies of this paper have been sent to England by Mr Green. The spot visited by the excursionists was about ten miles from the mouth of the crater. Mr Vaudry, an English traveller, had set out to reach the crater itself, and had not returned when the "Kamoi" left the Island.

"From the distance at which we observed it, about ten miles, the crater *appeared* to be circular, and perhaps 300 feet across. It may prove to be 500 or even 800 feet across. The rim of the crater is surrounded by cones of stones and scoriæ, these cones constantly varying in extent, now growing in size, and again all tumbling down. The lava does not run out from the side of the crater, like water from the side of a bowl, but is thrown up in continuous columns, like the geyser springs as represented in school geographies. At times this spouting appeared to be feeble; but generally, as if eager to escape from the pent-up bowels of the earth, it rose to a height nearly equal to the base of the crater. The columns and masses of lava thrown out were ever varying in form and height. Sometimes, when very active, a spire or cone of lava would shoot up like a rocket, or in the form of a huge pyramid, to a height nearly double the base of the crater. If the mouth of this be 500 feet across, the perpendicular column must be 800 to 1000 feet in height. Then, by watching it with a spy-glass, the columns would be seen to diverge, and fall in all manner of shapes, like a fountain.

"This part of the scene was one of true grandeur, no words can convey a full idea of it. The molten fiery redness of the lava, ever varying, ever changing its form, from the simple gurgling of a spring to the hugest fountain conceivable, is a scene, when viewed, that will be painted in all its splendour and magnificence on the memory of the observer till death. Large boulders of red-hot lava stone, weighing hundreds if not thousands of tons, thrown up with inconceivable power high above the liquid mass, could be seen occasionally falling outside, or on the rim of the crater, tumbling down the cones, and rolling over the precipice, remaining brilliant for a few moments, then becoming cooled and black, were lost among the mass of surrounding lava. The observer cannot help watching it with intense delight, the only drawback being the severe cold of the night."

A dense column of smoke is described as continually rising out of the crater, on the north side, in a continuous column perhaps 10,000 feet high. The observer watched very closely to notice whether any *steam* could be seen issuing either from the crater or any of the streams of lava, but could not discover any, except where the molten lava came in contact with trees and vegetable masses in its course.

"On leaving the crater, the lava-stream does not appear for some distance, say an eighth of a mile, as it has cut its way through a deep ravine or gulf, which hides it from the eye. The first that we see of the lava, after being thrown up in the crater, is its branching out into various streams some distance below the fountain-head. Instead of running in the large stream, it parts and divides into a great number, spreading out to five or six miles in width. For the first six miles from the crater the descent is very rapid, the flow of lava varies from four to ten miles an hour. But after it reaches the level plain the stream moves slower. Here the streams are not so numerous as higher up, there being a principal one, which varies and is very irregular, from an eighth to half a mile in width, though there are frequent branches running off from it. This principal stream reached the sea near Wainanalii on the 31st January, after a flow of eight days. When it reached the sea it spread out about half a mile in width. Some of the finest scenes of the flow were the cascades or falls formed in it before the stream reached the plain. There were several of them, and they appeared to be changing, and new ones formed in different localities as new streams were made. One, however, which appeared without change for two days, must have been 80 to 100 feet in height. First, there was a fall, then below were cascades or rapids. To watch this fall during the night, when the bright red-hot stream of lava was flowing over it at the rate of ten miles an hour, like water, was a scene never to be forgotten. On reaching the plain, where it is more level, the lava stream, of course, moves along more slowly and less divided than above. The stream which had run into the sea had apparently ceased flowing, and was cooled over, so that we crossed and re-crossed it in many places, and through the fissures we could see the molten lava with its red-hot glow. An intense heat issued out from them. In many places the surface was so hot that the soles of our shoes would have been burnt had we not kept in rapid motion.

"On the afternoon of our arrival at the encamping ground a new stream started some few miles below the crater, which had evidently been damned up by some obstruction, and came rushing down with tremendous noise and fury through the thick jungle which lay in its track, burning the cracking trees, and sending up for a time a thick smoke almost as dense as that from the crater. This stream, from the time it broke away from its embankment, moved along two miles an hour, till it reached the vicinity of our camp, when its progress was checked, and it moved not more than a quarter of a mile an hour. But it formed a grand sight. Here was a stream of lava rolling over the plain, twenty to twenty-five feet in height, and about an eighth of a mile in width, though its width varied a great deal, sometimes broader, sometimes narrower. It was, in fact, a mass or pile of red-hot stone, resembling a pile of coals on fire, borne along by the liquid lava stream underneath. As it moved slowly along, large red-hot boulders would roll down the sides, breaking into a thousand small stones, crushing and burning the trees which lay in the track."

"The poor inhabitants of Wainanalii, the name of the village where the fire reached the ocean, were aroused at the midnight hour by the hissing and roaring of the approaching fire, and but just in time to save themselves. Some of the houses of the inland portion of the village were partly surrounded before the inmates were aware of their danger. The village is, of course, all destroyed, and its pleasant little harbour filled up with lava."

Description of New Protozoa. By T. STRETHILL WRIGHT, M.D., Fellow of the Royal College of Physicians, Edinburgh.* With three Plates.

Description of Plate VII.

Fig. 1. *Lagotia producta*, group enlarged.
2. Single specimen of do.
3. Diagram showing section of tube—*a*, chitinous ribbon—*b* and *c*, internal and external fleshy coats.
4, 5, 6. Young of *L. producta* in various stages.
7. *Zooteirea religata*—*a*, extended—*b*, contracted.
8. *Corethria Sertulariæ*—*a*, "cushion"—*b*, "mop"—*c*, Gregarina-like appendage. 9. Summit of "mop"—*a*, external, and *b*, internal coat. 10. Summit of Gregarina-like appendage.
11. *Salpistes Mülleri*, —*a*, gelatinous lorica.

1. *Lagotia producta.* (Figs. 1–6.)

At our meeting of the 22d April 1857, I described *Lagotia viridis*, which I had discovered some time before, together with two other species, *Lagotia hyalina* and *atro-purpurea*. The present species, *Lagotia producta*, was found in great profusion in the tanks of Miss Gloag of Queensferry, in August last, and resembles *Lagotia viridis* in its general characters. It has the same long green body, surmounted by a horse-shoe-shaped or two-pronged rotatory organ, each prong formed of a membrane folded together, like the ear of a hare, and edged by a muscular band fringed with vibratile cilia. It inhabits also a similar flask-shaped cell, with bent neck and trumpet-shaped mouth. But the body of *Lagotia producta* is greatly prolonged in comparison with that of *Lagotia viridis;* the colour nearly black, and the gullet a wide sac, and not a spiral canal. In the present species, the neck of the cell has become a long wide tube, greatly disproportionate to the flask-shaped part from which it projects; the whole bearing some resemblance to a long jack-boot. A careful examination of the anatomy of the cell reveals a structure of singular interest and beauty. It consists of three elements—(1.) a horny or chitinous case, lined with (2.) a layer of dark green sarcode, and covered with (3.) a thin layer of colourless sarcode; so that the cell is not a mere extraneous house in which the animal lives, but is a quasi-bony framework, buried in an extension of the living flesh of the animal, and growing with its growth. And this, I believe, holds good with regard to the tubes of other genera of Vorticellina. It is especially to be seen in *Vagincola valvata*, a species which I have described to the Society, and in which the layer of sarcode lining the tube serves to support and partly to form a valve, which rises and falls with the movements of the animal within.

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* Communicated to the Royal Physical Society, November 24, 1858.

The chitinous case of *L. producta*, or that part of it corresponding to the body of the flask, resembles that of *Lagotia viridis;* but the neck or tube is composed of a spirally-wound ribbon of chitine, the upper edge of each spiral slightly overlapping the lower edge of the spiral arising above and within it, and being slightly everted. The edges have no chitinous connection with each other, but are retained in their places by their fleshy coverings. One might suppose that the tube was formed and gradually increased in length by a continual addition to the length of the spiral ribbon, and that with the increasing age of the animal the number of spiral turns would continually increase. Such, however, is not the case, as I shall proceed to show.

Mode of Reproduction in Lagotia.

As I was anxious to ascertain the mode of reproduction in Lagotia, I was kindly allowed to take away with me several of the stones from the aquarium. These were placed in a large glass vessel of sea-water, and careful daily search was made for some weeks to discover larvæ, but without success, although young Lagotias were beginning to attach themselves to the sides of the glass. At last, I discovered several dark green specks swimming in the water: these were caught one by one, placed in large flat cells, and covered by bell-glasses, to prevent evaporation, where they became developed into Lagotias.

The young of *Lagotia producta*, in its earliest stage (fig. 4), is a short cylindrical body, with rounded extremities of a dark sea-green colour. The surface of the body is marked by coarse striæ, each of which carries a fringe of long lashing cilia, by the aid of which the animal urges itself rapidly through the water, at the same time rotating on its axis. It quickly assumes a more lengthened form (fig. 5), the anterior extremity of the cylinder puts forth a fringe of long cilia, and the posterior extremity becomes pointed, while the cilia of the body become much diminished in size. A number of young at this stage were placed in the flat cells, and were found the next morning to have attached themselves to the surface of the water as to a solid substance, and to have developed their tubes with all the spiral rings complete, the imbrication of the rings being even more marked than in the adult specimens. The rudiments of the bilobed rotatory organ had also appeared (fig. 6); while the cilia of the body had still further decreased in size, although the striated texture of the surface was still strongly marked, and formed a beautiful object under strong microscopic power. In three or four days, the lobes of the rotatory organ and the general structure of the animal were completed, the young Lagotia had increased in size, and its tube had become more opaque.

The spiral structure of the tube appears to afford a provision for its growth similar to that found in the articulated shell of the Echinus. In *Lagotia producta*, increase in length of the tube would be effected by the deposition of chitine on both edges of the spiral ribbon, while increase in its calibre would take place by the gradual unrolling of the same. The chitine is probably secreted by the thick inner coat of the tube; while the external coat appears to act, like the "colletoderm" of zoophytes, as a cement for attaching the cell to the rocks. These young Protozoa frequently assembled in clusters, and secreted a quantity of "colletoderm," which glued all their cells into a single mass.

So far, the observation as to the reproduction of Lagotia is in some measure satisfactory; but it still remains to be discovered how the ciliated larvæ are produced. In *Epistylis nutans*, one of the Vorticellina to which Lagotia is allied, an encysting process takes place, according to Stein, by which the animal takes the form of an Acineta. The ciliated head is absorbed; the body is inclosed in a tough tunic; and numerous long capitate tentacles are put forth, which have the property of sucking tubes, and quickly absorb the fluid contents of any animalcule coming in contact with them. Within this Acineta body one or more ciliated embryos are formed, and successively given off, until the substance of the Acineta is entirely exhausted, and it becomes an empty sac. A similar transformation into the Acineta state has been noticed in Vaginicola, a still nearer ally of Lagotia. I have, however, not been able to detect any such change in the subject of this notice.

2. *Zooteirea religata.* (Fig. 7.)

This Protozoan is an inhabitant of deep water, and was dredged from the oyster-beds opposite to Newhaven. It was attached in considerable numbers to the concavity of the lower valve of an old oyster-shell, from whence it propagated itself to the sea-weeds of the vessel in which it was confined. Zooteirea may be briefly described as an Actinophrys mounted on a contractile stalk. Actinophrys, again, consists of a globular mass of sarcode, in which may be distinguished two tissues: an internal one, which I shall call "endosarc," enveloped by an external tissue, "ectosarc." The endosarc is dense, loaded with molecular matter and nutritive granules; the ectosarc transparent, and produced into tentacular appendages. Actinophrys has no mouth. Animalcules, seized by the tentacles, are drawn to the surface of the body, the soft sarcode of which becomes depressed, closes over them, and envelops them. They sink into the endosarc, and are absorbed. The endosarc is the *alimentary tissue;* probably also the reproductive tissue. The ectosarc exercises the prehensile function. The tentacular processes of Actinophrys are homologous with those prehensile processes of the "ectoderm" which I have described as existing in several classes of aquatic animals, and to which I have given the term "palpocils," a term which has lately been adopted by my friend Mr Gosse, in his interesting paper on *Sarcodyction catenata.* In Zooteirea, when expanded, the whole of the ectosarc is prolonged into long and exceedingly attenuated palpocils, until the animal assumes the appearance of a globular brush of spun glass mounted on a transparent stalk. When irritated, the animal slowly contracts its stalk until the body is brought close to the surface on which it is attached, and the palpocils are contracted to a mass of little nodules (fig. 7, *b*). The stalk is homogeneous, and is, as are the palpocils, a process of the ectosarc. A group of these animals form a very striking microscopic object when seen by the dark field illumination,—two cones or brushes of light appearing to issue from opposite sides of the body of each, and to pass round it in opposite directions when the mirror is moved. I have derived the name Zooteirea from Ζωοσ and τείρεοσ, *a star*, or rather *a constellation.*

3. *Corethria Sertulariæ.* (Figs. 8, 9, and 10.)

This remarkable animal has occurred plentifully during the last few summers on the *Sertularia pumila,* which grows at low-water mark near Granton. I have only found it in one locality, at the extremity of the first ridge of rocks which runs out into the sea west of the long breakwater. Although its anatomical structure is protozoan, it can be classed in no described family of Protozoa. It consists of three parts: 1*st,* An oblong cushion of opaque granular sarcode (fig. 8, *a*) attached to the corallum of the zoophyte, and sometimes containing a few vesicules. 2*d,* A long column attached to the cushion (*b*), bearing a brush of short tentacles. This column consists of two tissues; an outer coat thrown into numerous transverse folds or wrinkles (fig. 9, *a*), and an inner core displaying a faintly-marked longitudinal structure (*b*). At the top of the column this inner coat appears to terminate in a brush, or rather mop of from ten to more than forty tentacles (*c*), which have occasionally a slow and rather irregular waving motion, though they are generally at rest. 3*d*, There exists, but not invariably, a long, spindle-shaped, and rather curved process of a granular tissue, similar to that of the cushion (fig. 8, *c*), also attached by one extremity to the upper surface of that body, and having at its unattached extremity a clear space, which opens externally by a small oral aperture. This body is often absent, and I have seen it attached alone to the Sertularia. I am therefore inclined to consider it either a gemma or a parasite belonging to Gregarinæ. Although Corethria bears no resemblance in form to any known Protozoan, it has anatomically all the elemental tissues of an Actinophrys. Let us suppose an Actinophrys in which the ectosarc or prehensile tissue is segregated from the endosarc or nutritive tissue, the former, instead of forming a multitude of palpocils, being gathered together into a single large tentacle, surrounded in greater part by a wrinkled cuticle, and undergoing division at its summit into a number of palpocils. Such a structure I have observed in the compound palpocils situated on the tentacles, at the extremities of the rays of *Solaster papposa*, and such appears to be the structure of the "mop" of Corethria. Food taken by the palpocils would be transferred through the soft sarcode composing the centre of the pillar, and digested by the granular endosarc of the cushion below. I have seen the spores of Algæ thus absorbed into and pass through the tentacles of *Ephelota apiculosa*. A like observation has been recorded with regard to the tentacles of an Acineta. From κόρηθρον, a mop.

4. *On Salpistes (Stentor) Mülleri and castaneus.* (Fig. 11.)

In the last edition of Pritchard's "Infusorial Animalcules" it is stated that *Stentor Mülleri*, "when kept long in glass vessels, fasten themselves to the sides, form a slimy covering around them, and die;" and, further, that Ehrenberg had remarked that "they would gradually congregate, select some particular spot, and then attach themselves, evincing, as it were, not only a degree of sociality, but a mental activity." In their apprehension of these facts I believe the authors above quoted to be mistaken. It is well known that many aquatic animals have the power of secreting masses of viscid gelatinous matter, which does not readily undergo decomposition, to serve as a nidus for the protection of their ova. Thus the nudibranchiate and other molluscs deposit on stones and weeds long convoluted ribbons of clear firm jelly filled with their eggs, which remain therein until hatched. Many insects, aquatic in their earlier stages of existence, adopt the same mode of protecting their young. In the genera *Sertularia, Plumularia, Campanularia,* and *Laomedea,* species also are found in which the young undergo partial development whilst still contained in gelatinous cases attached to the exterior of the reproductive cells, as I have already described to the Society in the case of *Laomedea lacerata.* Other animals employ the same matter to form envelopes or loricæ, into which they can retire protected from harm. In this way is formed the "house" of *Appendicularia flabellum,* of which Mertens has given so marvellous an account, mistaking it for a respiratory organ. Amongst the Rotiferæ we find *Stephanoceros, Floscularia, Limnias, Melicerta,* and others, each living privately in its solitary abode, formed either of clear gelatine, or of the same substance strengthened with mud or other extraneous matters; while in the genus Conchilos a colony of animals unite their efforts to form a transparent globe, which is rapidly rolled through the water by a multitude of living wheels. Descending to the Protozoa, we may see *Ophrydium versatile,* an animal scarcely visible to the unassisted eye, attaching itself to our tanks by its little speck of jelly. It then immediately proceeds to multiply itself in geometrical proportion by fissure until masses are formed, which, under favourable circumstances, attain a diameter of three or four inches, and consist of aggregations of many thousands of zooids. In this Protozoan the division is not complete; the zooids are united by fine threads, which permeate the gelatinous mass, and are homologous with the stalks of

Epistylis. These threads also appear to have the property of transmitting nervous impressions through the whole mass of this compound animal, and rendering the movements of the associated zooids consentaneous.

When in England some years ago, I found in one of my fresh water aquariums a great number of specimens of *Stentor Mülleri*, each one of which was surrounded at its base by a flocculent deposit similar in structure to the lorica of *Ophrydium versatile*, and into which the animal could withdraw itself. At first I considered that this state was the result of disease, but further experience showed that the deposit was never absent. Many of the animals inhabited a tall gelatinous pillar, by which they raised themselves considerably above the surface on which they grew (fig. 11). Others by fissure had formed colonies, which were attached to the glass, or hung downwards while floating on the surface of the water; others, again, were swimming naked in search of sites for future erections; but no fixed animals were found to be destitute of a lorica. I have repeatedly met with this animal since, and always in the same loricated state.

In the summer of 1857, a small species of Stentor, of a deep chestnut colour, occurred in the pond of the Edinburgh Botanical Gardens, which is in the habit of secreting a lorica like that of *Stentor Mülleri*. This species, which I have called *castaneus*, selects the tips of the shoots of Myriophyllum for its abode, and glues all the opening leaflets together with a mass of jelly, from which the zooids protrude their wheel-bearing heads. The possession of a lorica removes *Stentores Mülleri* and *castaneus* from the family Vorticellina to that of Ophrydina, which (says Ehrenberg) "includes true Vorticellæ or Stentors inclosed in a gelatinous membranous little box" or shell. In the last family, a new genus will have to be formed for their reception, for which I propose the name of *Salpistes*, from σαλπιστὴσ, *a trumpeter.*

Since the above was written, my friend Mr Alder has informed me that he has also discovered *L. producta* near Tynemouth.

Observations on British Zoophytes. By T. STRETHILL WRIGHT, M.D., &c.

Description of Plates.

PLATE VIII.

Fig. 1. *Clavula Gossii*, proles *Turris neglectæ.*
2. *Hydra tuba* (*Strobila*) in various stages. 3. Corallum of same.
4. *Bimeria vestita*; *a*, single tentacle, the clothed portion studded with parasitic Algæ.
5. *Garveia nutans.*
6. *Coryne implexa.* 7. Thread-cells of do.

PLATE IX.

Fig. 1. *Goodsirea mirabilis.* 2. Thread-cells of do.
3. Marginal tubercle with its two tentacles.
4. Young of Cydippe.
5. *Eudendrium arbuscula*, stalked cluster of double spermatic sacs.
6. Section of double spermatic sac—*a*, tubercle containing, *c*, barbed thread-cells.
7. *Psuchastes glacialis.*

1. *On the Reproduction of Turris neglecta.*[*]

The only observations that we have as to the reproduction of the gymnopthalmatous Medusæ are those of Mr Gosse with regard to *Turris neglecta.* He is the pioneer who first actually witnessed, or rather caught a glimpse of, the reproduction of a hydroid zoophyte from a recognised species of Medusa. In September 1852, he saw the oval purple gemmules of *Turris neglecta* escaping from the walls of the ovaries, and dropping down to the bottom of the vessel in which they were confined, where they moved slowly about by means of their vibratile cilia. He placed a number of these gemmules in a properly-constructed cell, and, by watching them, ascertained the following facts:—“ The gemmule (says he) having adhered to the glass, grows out into a lengthened form, variously knotted and swollen, and frequently dividing into two branches, the whole adhering closely to the glass. After a day or two's growth in this manner, a perpendicular stem begins to shoot from some point of this creeping root, and soon separates into four straight, slender, slightly divergent tentacles, which shoot to a considerable length. The little creature is now a polyp of four tentacles." At this stage they all died, and he never succeeded in repeating his observations. In August last, I picked up at Queensferry a specimen of *Turris neglecta* laden with dark crimson ova. This prize I accommodated with a commodious apartment in which it might exercise the duties of maternity. After a weary delay of nearly a fortnight, the young made their appearance as dark crimson ciliated larvæ. These underwent the changes so well described by Mr Gosse; but instead of being destroyed by starvation in their infancy, the four-armed polyps underwent a further development into a zoophyte resembling *Clava repens* (fig. 1, Plate VIII.). The young of *Turris neglecta,* which I now place on the table, and to which I have given the name of *Clavula Gossii,* may be described as follows:—

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[*] Communicated to the Royal Physical Society, 24th November 1858.

Clavula Gossii (Proles *Turris neglectæ*). Polypary creeping, sheathed in a chitinous polypidom. Polyps minute, seated on short stalks, spindle-shaped, furnished with about twelve tentacles; upper row of tentacles long, filiform, four in number, erect; rest of tentacles scattered, shorter, inclined upwards; colour crimson.

2. *On the Development of Hippocrene (Bougainvillea) Britannica from Atractylis (Eudendrium) ramosa.**

This paper appeared as a note to Dr Wright's paper on Atractylis in the Number of this Journal for January 1859.

3. *On the Development of Hydra tuba (Strobila) from Chrysaora.*†

In September last, I extracted a larger number of young from the reproductive sacs of Chrysaora. The young in their first stage are (as has been repeatedly observed) swimming ciliated larvæ. The greater part of these attached themselves to the surface of the water, and hung downwards as globular sacs seated on long thin pedicles or stalks (Plate VIII., fig. 2). The pedicles were surrounded by a thick and very transparent gelatinous case, corallum, or polypidom. The globular sac acquired a mouth, and afterwards four, eight, sixteen tentacles successively. As the Hydra grew, it produced additional attachments from its body. The bases of these attachments in the fully-developed Hydra appeared as a number of closely-aggregated circles (fig. 3), in which the four tissues, colletoderm (*a*), corallum (*b*), ectoderm (*c*), and endoderm (*d*), could be distinctly made out in those specimens attached to surfaces of glass. It appears, from the above observations, that the *Hydra tuba* is not a naked polyp, as hitherto described.

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* Read to the Royal Physical Society, Nov. 24, 1858.

† Ibid.

4. *Coryne implexa* (Alder).*

Under the title of *Tubularia implexa*, my friend Mr Alder has described a zoophyte discovered by Mr R. Howse in 40 fathoms water, 30 miles from Holy Island. Mr Alder's description of it is as follows :—" *Tubularia implexa*.—Tubes small, very slender, generally more or less contorted below, smooth, wrinkled or regularly annulated beneath a smooth transparent epidermis ; slightly and subunilaterally branched ; the branches going off nearly at right angles to the stem, and a little constricted at their base; gregarious, forming a densely-tangled mass of half to three-quarters of an inch in height.

The polyp of this zoophyte had not been observed, for which reason Mr Alder considered that its claim to a place in the genus Tubularia could not be fixed very decidedly. Its most remarkable feature is the structure of the corallum or polypidom, which is divided into two coats, as in Plate VIII., fig. 6; a structure hitherto observed only in one other species, the *Campanularia caliculata* of Hincks. Mr Alder kindly sent me a specimen of his *Tubularia implexa*, which, after a careful examination I concluded to be, not a Tubularia, but a Coryne; and I wrote to Mr Alder to that effect. Fortunately, although the specimen was destitute of polyps, portions of the polypary or cœnosarc still remained, and I found in its tissues two kinds of thread-cells, the one oval, and containing a barbed dart (fig. 7, *a*), the other cylindrical, with almost truncated extremities, in which the thread was not conspicuous. (*b*), The first of these resembled very closely the oval, barbed thread-cells of *Coryne*

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* Read to the Royal Physical Society, 26th January 1858.

decipiens; while the second, although much larger, evidently corresponded to the long, slender thread-cells found on the body within the corallum, and especially in the tips of the growing shoots of the last named zoophyte.

The internal layer of the corallum is brown and of horny texture; the external coat colourless and membranous. The first is frequently annulated; the second not so, but is occasionally gathered in longitudinal folds. I am disposed to think that this coat is the "colletoderm," or glutinous covering of the corallum (in this species highly developed and indurated), separated from the inner coat by the action of the spirit in which the specimen was immersed. In all the Corynes I have examined, this "colletoderm" forms a thick layer over the corallum, especially in the neighbourhood of the polyp.

In September last, I found on Inch Garvie the beautiful large Coryne which I place on the table this evening; one of the polyps being shown at Plate VIII., fig. 6.

Coryne (margarica, mihi) implexa (Alder).

Corallum branched or creeping; composed of two coats, the inner coat horny, annulated at intervals; the outer coat membranous, smooth, longitudinally folded near the polyps. Body of the polyp cylindrical, much elongated; summit truncated, very transparent, of a pearly white colour; mouth surrounded by a dense white ring. Tentacles small and slender, very numerous. Thread-cells on tentacles oval, barbed; on the body of polyp, long, cylindrical. Both kinds of thread-cells within the corallum.

This zoophyte, as to its polypary, bears a close resemblance to the *Tubularia implexa* of Alder, and I have little doubt is identical with it; in which case *Tubularia implexa* is, as I suspected, a Coryne. It has the same double structure in the tube of the corallum, and the thread-cells, both in form and size, are identical. The polyp of the species is distinguished from all others of its genus which have come under my notice by the extreme transparency of its tissues, and the small size of its tentacles; in which last particular it resembles the *Coryne pelagica* of Alder, and even approaches *Myriothela artica.* The thread-cells on the tip of the capitate tentacle are of very small size, as in Myriothela, with the exception of one, or sometimes two, of the large cells which are found in the same situation in other species of Coryne.

5. *Bimeria vestita.** (Plate VIII, fig. 4.)

Polypary minute, very slender, branched, smooth, or wrinkled near the division of the branches, inclosed in a transparent horny corallum; polyps vase-shaped, destitute of proboscis; tentacles slender, alternate, as in Eüdendrium; corallum, body, mouth, and lower half of each of tentacles of polyp clothed in an opaque brown membrane; thread-cells inconspicuous.

This remarkable zoophyte first occurred to me on the Bimer Rock, near North Queensferry, in August last, and afterwards to Dr M'Bain and myself on Inch Garvie. It differs from all zoophytes hitherto described in being completely clothed—as the corallum, the bodies of its polyps, and part of their tentacles—in a thick, soft membrane, which appears to be formed of the glutinous "colletoderm" thickened by fine mud. The tentacles were frequently united together in pairs by the same substance. The unclothed half only of the tentacles was furnished with thread-cells. The bodies and clothed part of the tentacles were frequently studded with minute crimson Algæ (fig. 4, *a*), which in some cases almost concealed the polyps, but did not seem to exercise any deleterious influence on their health.

The male reproductive apparatus consisted of an ovate pedicled ectodermal sac (fig. 4, *b*), inclosing a linear unbranched process of the endoderm, as in Hydractinia, the whole inclosed by the horny corallum with its muddy covering. The female reproductive system was not discovered.

6. *Garveia nutans.* (Plate VIII., fig. 5.)†

Polypary inclosed in smooth or slightly-wrinkled corallum, creeping or forming a stem of many agglutinated tubes from which the polyp stems diverge as branches; polyps not retractile within the corallum, decumbent when contracted; tentacles about ten, thick, in a single row, not alternate; mouth not trumpet-shaped; colour of polyp vermilion and yellow; thread-cells inconspicuous.

This zoophyte, which occurs on Inch Garvie, is conspicuous for the singular colour of its polyps; the ectoderm being of a fine transparent yellow, the endoderm vermilion. Consequently the tentacles are yellow, while the body of the polyp is red. When irritated, as by being removed from the water and re-immersed, the zoophyte bends all its polyps downwards, like flowers drooping on their stalks.

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* Read to the Royal Physical Society, 26th January 1859. † Ibid.

The reproductive capsules (female), which I have only seen in a specimen after immersion in spirits, arise from the creeping polypary or the compound stem, and resemble in their external characters the female capsule of *Eudendrium rameum.*

This zoophyte was at first mistaken by me for an Eudendrium, but it differs from the latter genus in the following particulars:—In Garveia the body of the polyp is fusiform; in Eudendrium globular, with a trumpet-shaped expansible proboscis. In Garveia the tentacles are arranged in a single row; in Eudendrium also in a single row, but each alternate tentacle is elevated or depressed, so that they appear to be disposed in two rows. In Eudendrium the body of the polyp is studded by very large thread-cells; in Garveia these thread-cells are absent.

7. *Goodsirea mirabilis, an undescribed Gymnopthalmatous Medusa.*[*]

Three specimens of this medusa (Plate IX., fig. 1) were taken in the Firth, near Queensferry, in September last, one of which I placed on the table this evening. In general form it resembled the *Plancia gracilis* of Forbes; but it differs from that animal in the structure of its smaller tentacles, the absence of eye-specks, and the presence of auditory organs. The disc is hemispherical, depressed, in some specimens elevated in the centre, and about an inch in diameter. Its margin is furnished with two large colourless tentacles, which are capable of being produced to the length of about two and a-half inches. For about one-third of their length they are hollow, and permeated by the circulating fluid of the lateral canals. The proximal portion of the large tentacles is covered with long, narrow, curved thread-cells, arranged in clusters or cones, like the piled muskets of a regiment of soldiers (fig. 2, *a*, *b*). These, towards the distal ends, became mixed with large scattered thread-cells of an ovate or rather almond shape (fig. 2, *c*), in the interior of which a long, loosely coiled smooth thread is very distinctly visible. The tips of the large tentacles are almost entirely furnished with the last kind of thread-cell. In addition to the large tentacles, the margin of the disc bears ninety-six small tubercles, each of which is connected with two exceedingly minute and delicate tentacles, of very complicated structure. The tubercles themselves (fig. 3 *a*), are covered with the long, narrow thread-cells above mentioned. The proximal ends of the smaller tentacles, for about one-third their length, are destitute of thread-cells, but are furnished with very thick, short palpocils. This portion of the tentacle terminates in a knob or swelling, *b*, which is covered with exceedingly minute thread-cells ; then succeeds a portion formed of nucleated cells, joined end to end in a single row, which portion terminates in a long ovate head, *c*, closely set with the large almond-shaped thread-cells, which were found on the tips of the larger tentacles. All these tubercles and their tentacles are destitute of ocelli. Eight otolithic sacs are attached to the exterior of circular canal, each containing about four otoliths. The sub-umbrella is formed by four lateral canals, with their connecting membrane. Its upper part dips downward so as to form a funnel, from the end of which the peduncle or alimentary polyp is suspended. The peduncle, about an inch and a half in length, is very extensile, and of a greenish white colour. It is terminated by a quadrangular campanulate mouth. The peduncle in the female is rendered quadrangular by the four band-like ovaries, which passed along its whole length, and contained countless eggs. In the male it is cylindrical, and includes a mass of spermatozoa between its ectodermal and endodermal layers. The whole of the lateral and circular canals are powdered, as it were, with very minute dark purple pigment granules. When floating in the sea, or jerking itself along by the rapid strokes of its disc, this medusa is only rendered visible by the snake-line motions of its peduncle and tentacles. All the rest of its body is as transparent as glass. In a well-lighted jar of sea-water, the outer surface of the umbrella glows with tints of blue, purple, and amber, reflected from the thin ectodermal membrane, which covers the gelatinous umbrella. With regard to this gelatinous structure, I have come to the conclusion that it is not the homologue of the ectoderm of the polyp, but rather a true corallum homologous with the horny plate of Velella and the corallum of Hydractinia. Did it consist of ectodermal tissue, it would be rendered opaque by alcohol, which is not the case, as may be seen in the specimen on the table. In captivity these animals float near the bottom of the jar in which they are confined, supporting themselves on their tentacles and peduncle, with which they are constantly searching the bottom, as if for food. One of the females discharged a large number of ova, which were carefully preserved in a proper vivarium, and watched, but no farther development took place in them. I had denominated this animal *Goodsirea*, after Professor John Goodsir the distinguished anatomist.

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[*] Communicated to the Royal Physical Society on the 23d March 1859.

8. *Note on the Young of Cydippe, Beröe, and Alcinöe.**

Vast shoals of animals belonging to the above genera congregated about Queensferry in August last, and amongst the adults were found numerous young. The young of Cydippe (a species tinged as to the roots of their tentacular cirri with chestnut pigment) resembled in shape an unripe acorn (Pl. IX., fig. 4), about $\frac{1}{20}$th of an inch in length. The stomach was as yet external to the gelatinous mass of the body. It consisted of a muscular, laterally-compressed, and somewhat conical sac (*a*), opening at its apex by a very distensible mouth, and communicating below by a small orifice with a large cavity (*b*) in the gelatinous body. This cavity represents the water vascular system of canals in the adult. It was divided into two lateral chambers (*b*, *b*) by the large sacs (*c*) for the reception of the tentacles. A single very large otolithic sac (*d*) protruded from the body between the bases of the tentacular sacs. The ciliary bands were four in number. The very long tentacles were furnished with one or two cirri only.

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* Communicated to the Royal Physical Society, April 27, 1859.

The young of Alcinöe resembled in shape a Beröe, being destitute of swimming lobes. Ciliary bands four. The tentacles, microscopic in the adult, were capable in the young of being extended to twice the length of the body. They, as are those of the adult, were covered with globular thread-cells, from which was projected a fine straight thread.

The ovate eggs of *Beröe fulgens* were obtained from the parent. They were hatched in about four days. The young resembled in shape the adult. Ciliary bands four. I was not able to make out the internal structure in the young of either Beröe or Alcinöe.

9. *Eudendrium arbuscula* (mihi).

Polypary branched, forming a bushy tree of adnate stems. Branches ringed near their insertions. Polyp white, terminal on very slender and transparent branches, with trumpet-shaped proboscis and numerous alternate tentacles. Base of body surrounded by ring of large thread-cells. Reproductive capsules (male) moniliform, double, borne in clusters on short stems springing at right angles from branches. Summit of double capsule with a tubercle containing barbed thread-cells.

One specimen of this zoophyte was found at Queensferry in September last; it was about two inches high, and thickly clothed with snowy polyps. The bunches of sperm-capsules (Pl. IX., fig. 5) resembled those of *E. capillare* of Alder, and *Sertolara racemosa* of Cavolini. The summit of the distal sac of each sperm-capsule (Pl. IX., fig. 6) was crowned with a curious tubercle (*a*) containing numerous large thread-cells (*b*), the thread armed with four barbs. These differed in shape from the large unbarbed thread-cells found in the body of the polyps.

10. *Psuchastes** *glacialis* (mihi). (Pl. IX., fig. 7.)

Under this title I have noted and figured a minute Alcyonian zoophyte which I have twice obtained attached to stones in the Firth of Forth. The first specimen consisted of two polyps, the second only of one. The polyp is about ⅛th of an inch in length, and furnished with eight short pectinated tentacles. It is attached by a spreading base to the rock. The whole of the coverings of the body and tentacles are thickly crowded with ragged calcareous spiculæ, from which the animal derives a curiously rough and crystalline aspect.

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* From *ψύχαστης, one who cools himself.*

While noting this zoophyte, I would also draw attention to a gigantic concatenated Alcyonian zoophyte, covered with long, spindle-shaped, calcareous spiculæ, now in the Anatomical Museum of Edinburgh, labelled as a Zoanthus.

PROCEEDINGS OF SOCIETIES.

Royal Society of Edinburgh.

Monday, 3d January 1859.—Professor KELLAND, V.P., in the Chair.

The following Communications were read:—

1. Note on certain Vibrations produced by Electricity. By Professor Forbes.

"In the course of last summer (1858) I became acquainted with a phenomenon described by Mr Gore in the Philosophical Magazine for June (Supplement, p. 519), of the following nature:—A metal cylinder, supported on two metallic rods or rails, the latter being in connection respectively with the poles of a battery, revolves in either direction, at will, under the action of an electric current copious in quantity. Also continuous rotation of a light copper ball, supported on two circular metallic rails, takes place in either direction at pleasure, depending on the first impulse. It appeared to me very probable that this interesting fact might be applied to explain what is still obscure in the experiment on heated metals, generally known as the "Trevelyan Experiment," described by Mr Trevelyan, in the "Edinburgh Transactions," vol. xii., where there is also a paper by myself on the same subject. With a view to elucidate the experiment, I had Mr Gore's circular railway and ball constructed some months since by Mr Kemp. I had not an opportunity of seeing it tried until 19th October, when I found it to answer well, with four Bunsen's pairs connected for quantity. The same day, in Mr Kemp's laboratory, I laid a brass "Trevelyan" bar or rocker on the edge of the brass plate, forming the outer rail of Mr Gore's machine, and connecting the rail with one pole of the same battery, and the bar (by means of a globule of mercury inserted in a cavity in its upper surface) with the other, energetic vibrations commenced quite resembling those occasioned by heat in the ordinary form of the experiment on a leaden support.

"I have since found, among other results,—1. That the vibration goes on in whichever direction the electric current passes. [At first I thought that there was a superior effect when the current passed from a good to an imperfect conductor, but this has not been confirmed, as far at least as I have gone.] 2. The vibrations take place both between metals of the same kind and heterogeneous metals. 3. When heat is applied to a brass bar vibrating on cold lead, and then electricity is applied as before, the effects are superadded to one another whichever way the current passes, the vibrations becoming more energetic, and if there be a musical note it becomes graver [owing, it is assumed, to the increased arc of vibration]. 4. When a bar of brass was placed so as to vibrate on two parallel upright plates, also of brass, respectively connected with the poles of a battery, the vibrations continued when the whole was immersed partly or wholly in water, and even when *flooded* by a powerful continued stream of cold water from a five-eighth inch pipe under considerable pressure. From this experiment I conclude that the effect of the heat developed by the electrical current in the thin upright plates may be fairly considered to be reduced so low as to be incapable of producing a sensible result (if such were ever the case). Indeed, allowing for the resistance and friction of the water tending to diminish the vibration, there is no ground for thinking that the action was less energetic in the one case than in the other. It is consequently reasonable to conclude that the effect in question is due to the repulsive action of the electricity in passing from one conducting body to another, and not to its effect in producing expansion.

"Now this is precisely the effect which I attributed to heat in the paper of 1833 already referred to. I therefore consider it a strong confirmation of the opinion I then expressed, from which I have never swerved, although it has not in general been received with much favour. The importance which I attach to this new confirmation, and the suggestiveness of Mr Gore's experiment on the rolling-ball, will be judged of from the fact, that in 1833, or earlier, I had an apparatus made, consisting of a bar resembling Mr Trevelyan's, but longitudinally divided by a non-conducting partition, while the two conducting sides were furnished with mercury cups for connecting them with the poles of a battery, the circuit being completed through the metallic base. The instrument exists, or existed a few years ago, though I am at present unable to find it. As well as I recollect, it was tried with an old-fashioned Cruickshank's battery of fifty pairs, without success. Indeed, I now find that, even with modern appliances, the experiment does not succeed when the circuit is only closed whilst *both* points of bearing of the rocker touch the mass or support.

"Since the date of the preceding notice (which was prepared for being laid on the table of the Royal Society at their meeting on the 20th ult.), I have continued and extended these experiments. As they are still in progress, I will content myself with mentioning two results as worthy of notice. I have obtained very active vibrations of carbon (such as is used in one of the elements of Bunsen's battery) resting upon brass, and also when it rests upon two pieces of carbon connected with the terminals of a battery. For this purpose, a battery having a certain amount of intensity is requisite in order to overcome the resistance of carbon as a conductor, but the vibrations are most energetic. The extremely small expansion which takes place in carbon by heat is another argument against that view of the Trevelyan experiment. The other experiment to which I refer is, that bismuth (and perhaps other metals) are not merely inactive as vibrators with any electric power which I have used, but the passage of electricity through them appears to have a *quelling* power which brings the rocker to instantaneous rest; yet bismuth permits a far freer passage of electricity than carbon: in one experiment I found that sixteen times as much was conducted. Something analogous was formerly observed by me in connection with heat applied to bismuth. I am now attempting to investigate the subject farther by experiment.

2. On the Temperature of the Sea around the Coasts of Scotland during the years 1857 and 1858; and the bearing of the facts on the Gulf-Stream Theory. By James Stark, M.D., F.R.S.E., &c.

Monday, 17*th January* 1859.—PROFESSOR CHRISTISON, V.P., in the Chair.

The following Communications were read:—

1. Notice of a Shower of "a Sulphurous Substance" (so-called), which fell in Inverness-shire in June 1858. By John Davy, M.D., F.R.S., London and Edinburgh, &c.

This shower took place about the 10th of June. The following account of it is from the *Inverness Courier;* and, as showing the interest the phenomenon excited, it was republished in most of the English papers. The copy I give is from the *Spectator* of the 3d July.

"After the late thunder-storm, a deposit resembling sulphur was observed in several places in this neighbourhood (Inverness). At Freeburn it lay on the road and grass in some places to a depth of nearly half an inch. At Craigton Cottage, near Kissock, the deposit was observed on the top of water caught in a cask from the roof of the house, like a thick cream. The sulphurous substance was skimmed off, and dried in a piece of flannel. When dry it was a fine powder, and when thrown into the fire ignited exactly like gunpowder, making a slight fizzing noise. Unfortunately none was preserved beyond what was experimented on in this way. A boat at Craigton was powdered all over with the same substance; and a countryman living on the heights near Kilmuir says, that near his house, in the space of what an ordinary washing-tub would cover, he could lift the powder with a spoon. The heavy rains have since washed it all away."

This account, so far as mere appearances are concerned, I believe to be trustworthy; I can in part confirm it, as it happened that on the very day, the 10th of June, I was *en route* for Inverness by the mail, proceeding by the upper Highland road. Just after crossing the Spey at Kingussie, the substance in question was seen, both on the road and by the road-side. It was in such abundance that the guard had no difficulty in collecting a quantity of it during the few seconds that the coach was stopped for the purpose. Of what was then gathered I obtained a portion, which, on my return home, I examined.

The results, as might be expected, proved that the substance was not sulphur, nor of a sulphureous nature, but a vegetable matter, the pollen of the fir—*Pinus sylvestris*, a tree of which there are extensive forests on the banks of the Spey. Before the blow-pipe, it burnt with flame, without the slightest sulphureous odour; and left a little charcoal, which, when consumed, yielded a minute quantity of ash, possessed of an alkaline reaction. When first inflamed it made no such noise as that mentioned in the newspaper; nor in burning did it in the least resemble gunpowder. If thrown on the fire before it was quite dry, the noise described in that notice might have been owing to the sudden conversion of the moisture into steam, producing a slight crepitation. Under the microscope it was found to consist of grains, some of a spherical form, others, and those of largest size, of the same form, with, as it were, a lateral addition, as if a smaller grain were attached, or as if the pollen grain had been ruptured at opposite points, and a protrusion of some of its contents had taken place.* The diameter of a single grain was about $\frac{1}{1070}$th of an inch. The central part of those supposed to be ruptured was commonly transparent, and exhibited a delicate granular structure, which was rendered slightly brown by the action of iodine. The lateral protuberances were more or less opaque.

Comparing the substance under consideration with the pollen of the Scotch fir, taken fresh from the tree, I find there is a perfect resemblance in form and colour and other properties. Further, in corroboration, it may be mentioned, that at the time of the occurrence of the shower, this fir was in flower in the Highlands very generally; and it is well known that when ripe, as it was at the time specified, it is apt to be shaken off by gusts of wind and rain in extraordinary quantities, so as to produce what have been called "sulphur-showers." By Sir John Richardson I have been informed that it is not uncommon to see the surface of the great lakes in Canada covered with a thick scum of the same kind in the vicinity of pine forests.

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* In the pollen of the Scotch fir we find, that by the increase of the intine the extine is separated into two hemispherical portions, marked by the dark spaces at each end of the grains.—*Edit. Ed. Phil. Jour.*

What renders the event, as it took place in the Highlands, remarkable, and no doubt rare there, is the extent of ground over which the pollen fell, and the quantity of it that was deposited.* It is worthy of notice, that this year the flowering of all our plants has been astonishingly abundant. This circumstance may account for the quantity; and the rareness of the phenomenon may be owing to the circumstances essential to its taking place seldom coming together, such as the incident of a thunder-storm with rain just at the time that the pollen is ripe for dispersion.

In the annals of the dark ages showers are recorded of a singular kind: in those of Ireland, for instance, as detailed in the last valuable census of that country, mention is made of those "of blood," of "a butter-like substance," &c.; and, analogous to that in Inverness-shire, "of a shower of a yellowish substance which resembled brimstone." This last mentioned is recorded as having "fallen in and about the town of Doneraile," in 1748.

2. Some Remarks on the Roman Edition of the Vatican Manuscript. By the Rev. Dr Robert Lee.

3. On the Constitution of Flame. By Mr Swan.

In this communication the author discusses the theory of the constitution of flame advanced by Professor Draper of New York, in his paper "On the Production of Light by Chemical Action," which appeared in the *Philosophical Magazine* for 1848.

Professor Draper refers to experiments which prove that the higher the temperature of an incandescent body, the more refrangible are the rays of light emitted by it. He assumes that the temperature of the outer portions of a flame is greater than that of the interior regions; for outside there is a better supply of oxygen, and hence a more intense combustion is maintained. He thence argues that a flame must consist of a series of layers of different colours following the order of tints in the prismatic spectrum, the red being innermost and the violet outermost. Professor Draper conceives that he has demonstrated by experiment that such a structure actually exists in flame.

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* The names of the places given in the newspapers where the pollen was observed denote, it will probably be admitted, the great extent of surface over which the shower spread. Craigton Cottage and Kilmuir are, I am informed, two miles from Inverness, about eighteen miles from Freeburn, and Kilmuir is about forty-four miles from Kingussie,—that is by the mail-road, but no more than thirty-three in a straight line due south.

Mr Swan, on a careful examination of various flames conducted essentially according to Professor Draper's methods of observation, with the exception of one particular in which that observer's process seemed objectionable, completely failed to verify his results; and on the following grounds believes his views regarding the constitution of flame to be erroneous:—

1*st*, Professor Draper's method of observation, so far as it can be gathered from his somewhat imperfect account of it, involves an error in principle calculated to lead to fallacious results.

2*d*, His theory is founded on the quite gratuitous assumption of a great diversity of temperature between portions of a flame so closely contiguous, as to render the existence of such diversity of temperature highly improbable.

3*d*, Even although great diversity of temperature did exist in different portions of a flame, there is no reason to believe that it would give rise to a series of layers of different colours. As the temperature of an incandescent body is raised, rays of continually higher refrangibility are, doubtless, emitted; but these are in every case accompanied by rays of *low* refrangibility. From this it follows that the outer regions of a flame, however high their temperature, will not yield *exclusively* the extreme violet rays of the spectrum, as Professor Draper supposes, but will equally emit the extreme *red* rays. The inference, therefore, regarding the colours of the different regions of a flame which Professor Draper has drawn from their assumed diversity of temperature, is obviously inadmissible.

Monday, 7th February 1859.—PROFESSOR KELLAND, V.P., in the Chair.

The following Communications were read:—

1. Biographical Memoir of the late Dr D. Skene of Aberdeen. By Alex. Thomson, Esq. of Banchory.*

2. On a new Arrow-Poison from China. By Dr Christison.

In a newspaper printed at Shanghae, in the spring of 1857, a wonderful account was given of a poison, which was said to be employed in the interior of China for destroying the largest animals. Instant death was said to be produced when an animal was struck in the trunk of the body with an arrow poisoned with it. Such was its potency, according to the opinion of the Chinese, that a scheme was said to have been set on foot for destroying the British army during the late war, by bringing down to Canton the natives who were in the practice of using it. But the scheme was frustrated by peace being unfortunately proclaimed too soon.

The poison, and apparently the plant also, are known by the Chinese name of *Wu-Tsau*, or Tiger-poison. The author received very lately from Dr Macgowan, an American physician residing at

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* This Memoir will be inserted in the next No. of this Journal.

Shanghae, a specimen of the poison, and of the root of the plant from which it is prepared. The root presents all the characters of an *Aconitum* on a very small scale. This corresponds with the conclusion to be drawn from the characters of a few leaves which were also sent, and which scarcely differ from those of *Aconitum ferox.* A farther proof is, that the root produces in an intense degree the very singular combination of numbness and tingling, which is occasioned by chewing the root of any of the active aconites known in Europe, such as *A. Napellus, ferox, sinense, uncinatum.* The poison itself, contained in a little porcelain bottle, is obviously a very well prepared extract; and if not entirely composed of the extract of the wu-tsau root, at all events must contain it largely, for a very minute quantity produces the most intense tingling and numbness of the tongue and lips after it is chewed.

There can be no doubt, therefore, that the wu-tsau poison must be extremely energetic. But the author objected to the admission that either this or any other arrow-poison can produce instant death, as is often stated by travellers. Every poison, however energetic, must be absorbed into the blood before it can act. Even from a wound, absorption cannot take place suddenly. Some time is required before enough can enter the blood. When death takes place instantly, the cause must be the mechanical violence inflicted by the arrow. The author exhibited various poison-arrows used in different parts of the world, which were adequate to occasion most deadly wounds if they struck the trunk of the body over an important organ; and he also showed that even the little slender wooden poison-darts, used in some parts of the world for destroying birds and small animals, by being shot from a blowing-tube, may be easily projected with a force amply sufficient to kill a small bird or animal by the violence inflicted, apart from the more tardy deleterious influence exerted by the poison.

3. On the Connection between Temperature and Electrical Resistance in the simple Metals. By Balfour Stewart, Esq.

About a fortnight since I mentioned to Professor Forbes that the resistances of the simple metals to the passage of electricity seemed to be very nearly in proportion to their absolute temperature, this relation being especially manifest for those values of the resistances determined by M. Arndtsen.

Professor Forbes informed me that this coincidence had already been observed by Professor Clausius, and that an abstract of his paper was given in the *Philosophical Magazine* for November last. On referring to Professor Clausius's original paper, it would seem that the coincidence had suggested itself to him as a remarkable similarity occurring between the rate of increase (due to temperature) of the electrical resistance of those metals, and that of the volume of a gas under constant pressure. It would seem that the fact was unconnected in his mind with any theoretical considerations.

As, however, I was led to look for and remark the coincidence by theoretical considerations, perhaps this Society will permit me to lay before them a short statement of my views.

The passage of electricity along a wire is generally viewed in the following light:—The free electricity of one particle decomposes the electricity of the particle next it; the two electricities then combine by sparking across the interval; and the same thing is renewed over and over again with extreme rapidity. Now, whether electricity be viewed as a substance or a motion, we may suppose that a polarized state of particles is alternately produced and destroyed with great rapidity while an electrical current passes along a conducting wire.

And by a polarized state of particles we would mean a peculiar disposition with respect to the direction in which the electricity is travelling, of the *matter*, or it may be *motion*, of the particles of the substance.

Again, an idea very generally entertained with regard to heat is, that it consists in a vertical or rotatory motion of the particles of a substance.

Now with only very general ideas regarding the mode of electrical conduction and the nature of electricity, we may suppose that the polarization which conduction demands will be resisted in proportion to the rotatory energy of the particle; just as a rapidly rotating top or cylinder would resist any attempt to change its plane of motion.

The resistance of a particle to electrical polarization would, therefore, be in proportion to its rotatory *vis viva*—viz., its absolute temperature. This only holds with regard to simple bodies; in compound bodies the passage of electricity may be supposed to be a more complicated phenomenon.

Monday, 21*st February* 1859.—Professor CHRISTISON, V.P., in the Chair.

The following Communications were read:—

1. Remarks on the Behaviour of Mercury as an Electrode. By T. Strethill Wright, M.D., &c. Communicated by William Swan, Esq. The experiments were shown.

In this paper the author described numerous experiments, serving to show the modification which occurred in the capillary attraction between mercury on the one hand, and various saline and acid fluids forming parts of active voltaic circles on the other. He also described and showed to the Society the undulatory motions which he had observed in mercury, when forming the cathode of a constant voltaic current in solutions of chloride of sodium, containing small quantities of sulphuric acid.

2. On the Natural History of the Herring. By J. M. Mitchell, Esq. Communicated by Dr Allman.

Before entering on the details of the natural history of the herring, the author points out the great value of the herring-fishery to the maritime nations of Europe; and quotes various scientific authorities to show, that the herring is superior in economical importance to every other fish. Thus Cuvier, in his work on fishes, edited by Professor Valenciennes, says,—Les grands politiques, les plus habiles economistes ont vu dans la peche du hareng la plus importante des expeditions maritimes."

Such views have led the British, Dutch, Swedish, and Norwegian governments, to inquire at present into the natural history, and to legislate regarding the fishery of the herring. The author has described the principal steps taken by these nations, and has given important statistical details of the British herring fishery, showing that fish, to the value of upwards of a million sterling, are annually taken on our coasts.

The high value of the fishery, not only in promoting the welfare of a large portion of our population, but in producing a strong, hardy, and industrious race of fishermen, most valuable to such a maritime nation as Britain, is next referred to.

The author then points out various errors regarding the herring which have been committed in works of high authority, such as Cuvier's work on fishes, already referred to, M'Culloch's Dictionary of Commerce, and the last edition of the Encyclopædia Britannica. He conceives that he has solved the doubtful questions regarding the natural history of the herring,—an object of the greatest importance, when we consider the high economical value of the fishery. He also points out several new and important facts regarding the appearance of the fish on our coasts. Among others, that the herring swims nearest the surface in dark and wild weather; and nearer the bottom when the weather is bright and cold.

He next enters on the details of the natural history of the herring, describing its characteristics and its distinctive difference from other fishes of its class. The important question of its food is elaborately examined; and it is shown, as stated to the author by Agassiz, that the herring does not confine itself to one species of food, namely that the food usually consists of minute crustacea; but during the spawning season it feeds on sand-eels, the fry of various fishes, and even its own spawn.

The author has ascertained a new and important fact from personal observation, regarding the cohesion of the spawn, and the power of adhering strongly to substances on which it may be placed, which only takes place on the fecundation of the roe by the milt.

Many writers reiterate the opinion, that the herring is a native of the distant northern seas. This the author shows to be an error, proving that the fish is a permanent inhabitant of our coasts.

He, for the first time, gives a complete description of the visits of the herring, or its geographical and chronological distribution over the surface of the globe, so far as is known; and his work is the first and only one which exhausts the difficult questions which have hitherto arisen regarding the most valuable and important fish which the bounty of Providence sends to supply food for the human race.

Monday, 7th March 1859.—PROFESSOR KELLAND, V.P., in the Chair.

The following Communications were read:—

1. Some Inquiries concerning Terrestrial Temperature. By Professor J. D. Forbes.

In this paper the writer starts by assuming Dove's Temperatures for the mean of every 10th parallel of latitude, from 75° N. to 40° S., to be correct. His object is to inquire how far the influence of the proportion of land and water in modifying the annual temperature of a given parallel is susceptible of being reduced to a formula.

The amount of land and water in different latitudes is first tabulated. It is then shown, from an examination of the ordinary isothermal curves, and also from Dove's charts of the "Thermic Anomaly," that the influence of land is to exalt the temperature of lower latitudes, and to depress that of higher latitudes. About the latitude of 42° or 43° this influence is *nil;* and the temperature of that parallel is independent of the proportions of land and water. For convenience of calculation, however, the latitude of 45° is assumed as the one free from this anomaly.

The writer shows that the decrement of temperature along an oceanic meridian (that of Greenwich, for example) proceeds nearly as the simple cosine of the latitude (which is Sir D. Brewster's formula); while along a continental meridian (one passing through Siberia) it is more nearly as the square of the cosine (the law of Mayer).

Hence it is argued that the temperature of any parallel may probably be represented by a formula containing (1) a constant; (2) a term varying with a power of the cosine not differing much from unity; (3) a term for the effect of land, containing as a factor the proportion of land on the parallel, and also the factor cos. $\overline{2 \text{ lat.}}$, which renders it additive below 45°, and subtractive afterwards.

Considering, first, the northern hemisphere alone, the constants are introduced into such a formula by a comparison of the observed temperatures of latitudes 0°, 30°, 50°, and 70°, with the following result:—

$$T_\lambda = 12^\circ{\cdot}5 + 59^\circ{\cdot}2 \cos. \tfrac{5}{4}\lambda + 38^\circ{\cdot}1\, L' \cos. \overline{2\lambda}$$

where T_λ is the temperature of latitude λ on Fahrenheit's scale, and L' the effective proportion of land in the circumference of that parallel.

The extension of the formula to the southern hemisphere is shown to give satisfactory results, although all merely *empirical* formulæ for the northern hemisphere altogether fail in this case.

Some other and independent confirmations of the formula are then adduced.

Supposing the globe to be entirely composed of land, or entirely of water, the temperature of any parallel may be deduced from the formula. In the case of a globe all land, the Equatorial temperature would be about 110°, and the Polar temperature about — 26°; whereas, on an aqueous sphere, the former would be about 72°, and the latter + 12°.

2. On the Spermogones and Pycnides of Lichens. By W. Lauder Lindsay, M.D., F.L.S. Communicated by Prof. J. H. Balfour.

The researches contained in the author's memoir, of which the following is a brief abstract, extended over a period of several years, and are based on careful microscopic examination of several thousand specimens of lichens from every part of the known world. The author examined the lichens of the Hookerian Herbarium at Kew, which contains an unrivalled series of specimens collected by the various surveying and exploring expeditions of the British Government, as well as by all the more distinguished modern British travellers. This collection is further valuable, from containing authentic specimens collected and named by Borrer, Turner, Hooker, Brodie, Carmichael, Babington, and other distinguished British lichenologists, as well as by Acharius, Schærer, Swartz, and other continental authors. Access was also had to the Menziesian Herbarium in the University of Edinburgh, the Herbaria of the said University, of the Botanical Society of Edinburgh, of the Museum of Irish Industry, Dublin, and of Dr Mackay of Dublin, which last contains authentic specimens of the lichens described by Dr Taylor in the "Flora Hibernica." The fasciculi of dried specimens published by Schærer, Hepp, Leighton, and others, as well as large numbers of specimens gathered in, and sent from, various parts of England, Wales, Scotland, and Ireland, were also examined. Besides the examination of dried specimens in herbaria, the author studied lichens extensively in their native habitats on the mountains of Scotland and Norway, and elsewhere.

With the exception of two short memoirs,* already published by the author, the researches in question are the first that have been made on the subject under review in this country. The memoir is essentially organographic, and gives full details of the characters of the spermogones and pycnides of the higher lichens,—that section, namely, which comprises fruticulose, filamentous, and foliaceous species. The genera, whose spermogones and pycnides are described, are:—

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* 1. "Monograph of the Genus *Abrothallus*." Quarterly Journal of Microscopical Science, Jan. 1857. Transactions of British Association for 1856. Botanische Zeitung, Dec. 25, 1857.

2. "On the Structure of *Lecidea lugubris*." Quart. Journ. of Microsc. Science, July 1857.

1. *Usnea*, Hoffm.
2. *Neuropogon*, Nees and Flot.
3. *Chlorea*, Nyl.
4. *Alectoria*, Ach.
5. *Evernia*, Ach.
6. *Dufourea*, Ach.
7. *Dactylina*, Ach.
8. *Ramalina*, Ach.
9. *Roccella*, Bauh.
10. *Thamnolia*, Ach.
11. *Sphærophoron*, Pers.
12. *Acroscyphus*, Lév.
13. *Stereocaulon*, Schreb.
14. *Bæomyces*, Pers.
15. *Cladonia*, Hoffm.
16. *Umbilicaria*, Hoffm.
17. *Cetraria*, Ach.
18. *Platysma*, Hoffm.
19. *Nephromium*, Nyl.
20. *Peltigera*, Hoffm.
21. *Solorina*, Ach.
22. *Sticta*, Ach.
23. *Ricasolia*, DN.
24. *Parmelia*, Ach.
25. *Physcia*, Fr.
26. *Pyxine*, Fr.
27. *Psoroma*, Fr.
28. *Pannaria*, Del.
29. *Coccocarpia*, Pers.
30. *Amphiloma*, Fr.
31. *Squamaria*, DC.
32. *Placodium*, DC.
33. *Ephebe*, Fr.
34. *Lichina*, Ag.
35. *Synalissa*, DR.
36. *Omphalaria*, DR. and Mont.
37. *Collema*, Ach.
38. *Leptogium*, Fr.
39. *Obryzum*, Wallr.[†]

The crustaceous section of the lichen family has been examined by the author in a similar manner; but the description of the pycnides and spermogones of the lower lichens is reserved for a separate memoir. Neither does he profess to enter upon the *questio vexata* of the physiology of reproduction in lichens generally. To this subject also he proposes devoting a special memoir. The memoir contains, *inter alia*, descriptions of the spermogones, or pycnides, of many species, in which they either have not hitherto been found, or in which they are very rare and difficult of discovery. Such are the spermogones of *Usnea barbata*, *Thamnolia vermicularis*, *Neuropogon melaxanthus*, *Alectoria jubata* and *A. Taylori*, *Evernia furfuracea*, *Lecanora tartarea*, &c. The letter-press of the memoir is accompanied by 16 quarto plates of coloured drawings—amounting to several hundreds—and by specimens of lichens, bearing spermogones or pycnides, amounting to about 140.

Among the more interesting or important general facts brought out by the memoir, may be enumerated the following:—

1. In addition to the sporiferous organs,—so long familiar to

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[†] The nomenclature and arrangement followed here and in the Memoir is that of Dr Nylander of Paris, as given in his "Synopsis methodica Lichenum omnium hucusque cognitorum." Paris, 1858. Pp. 65.

botanists,—called *Apothecia*, lichens possess more or less microscopically minute organs, called *Spermogones*. The *latter* organs the author has found alike in species from arctic and antarctic, temperate and equatorial, regions.

2. The spermogones of lichens may concisely be described as follows:—In *form* they are usually more or less spherical or oval, appearing on the surface of the thallus as punctiform or papillæform, wart-like or barrel-shaped, bodies. In *colour* they are usually blackish, brownish, or of divers colours. In *site* they are usually scattered over particular portions of the thallus—seldom generally over its whole surface; they are usually more or less immersed in the tissues of the thallus, sometimes they are sessile on its surface, or on the apices of its ramifications, when it is erect and fruticulose. In *size* they are seldom sufficiently large to be visible to the naked eye, and are frequently so minute that they can, with difficulty, be recognised even with the aid of a good lens. In the latter case, especially, it is advisable or necessary to moisten the thallus in order to render them prominent from the contrast of colour or surface. They consist of a capsule, enclosing a cavity, that opens to the surface by a minute pore or ostiole, which is generally of a darker colour than the said capsule. From the inner wall or surface of the capsule project, convergently into the cavity, a series of filaments, closely aggregated, called *Sterigmata*. These are either simple, formed of a single elongated cell,—or compound, made up of a series of cells, varying in size and form, superimposed the one upon the other. They produce from their apices, when simple,—from apices and sides, when compound or articulated,—a succession of very minute, solid, homogeneous corpuscles, called *Spermatia*. Just as the importance of the apothecia resides in the spores, so the importance of the spermogones resides in the spermatia. These corpuscles are destitute of any essentially vital motions, though they frequently, from their great tenuity, exhibit Brownian or molecular movements. Nor are they provided with any such appendages as cilia.

3. Spermogones usually occur on the same plant or specimen which bears apothecia: sometimes, however, only on barren plants or specimens. Hence, in regard to apothecia and spermogones, lichens have been described by continental authors as monœcious and diœcious. Again, spermogones occur in some species which never bear apothecia, as in *Thamnolia vermicularis*.

4. Spermogoniferous states of many species are what were regarded by the older lichenologists as separate varieties, species, or even genera, *e. g.*, vars. *encausta* and *vittata* of *Parmelia physodes*, and var. *denticulata* of *Platysma nivalis*. The discovery, therefore, of spermogones and pycnides simplifies lichenology by abolishing certain genera, species, and varieties, and materially reducing the number of names, *e. g.* the old genera *Cliostomum*, *Thrombium*, and

Pyrenothea, being found to consist wholly of spermogoniferous states of more familiar lichens, have been abolished.

5. To a certain extent, or with trifling exceptions, just as certain families or genera of lichens are characterised by apothecia and spores of a particular kind, so they are also characterised frequently by spermogones, sterigmata, and spermatia of a particular kind. Thus *Usnea* and *Ramalina* have wart-like spermogones of the same colour with the thallus, and inconspicuous in consequence thereof. The spermogones of *Cladonia* are barrel-shaped, deep brown, and easily visible. Those of *Parmelia* are mostly punctiform, immersed, black or brown, very minute and crowded; those of *Physcia* are mostly papillæform; and those of *Collema* and *Leptogium* discoid, and of a pale brown or yellow colour. In *Sticta*, *Ricasolia*, *Umbilicaria*, *Collema*, *Leptogium*, *Thamnolia*, *Coccocarpia*, and *Placodium*, the sterigmata are articulated, consisting of numerous, short, broadish, frequently thick-walled, cells. They are also articulated or compound,—but the component cellules are few, longish, and delicate,—in *Usnea*, *Neuropogon*, *Parmelia*, *Evernia*, and *Platysma*. They are simple and filiform in *Ramalina*, *Cladonia*, *Ephebe*, and *Lichina*. The spermatia of *Cladonia* and *Roccella* are curved or sickle-shaped. Those of *Squamaria* are very long, slender, and twisted or curved. Those of *Collema*, *Leptogium*, and *Umbilicaria* are short, straight rods; while those of *Ramalina*, *Ephebe*, and *Lichina*, are oblong or oval-oblong.

6. The spermogones sometimes of themselves yield important characters for classification. For instance, the spermogones, sterigmata, and spermatia of *Thamnolia vermicularis* are sufficient of themselves to separate this puzzling lichen from the genus *Cladonia*, in which it has hitherto been almost uniformly placed.

7. Spermogones frequently outwardly resemble, and are therefore apt to be confounded with—

a. Nascent apothecia, as in some *Ricasolias;*
b. Pycnides, described below;
c. Minute *Verrucarias;* or
d. Minute parasitic fungi, chiefly of the genus *Sphæria.*

For instance, the author frequently receives from correspondents as *new Verrucarias* what prove to be merely spermogoniferous states of other lichens. From all the bodies above named, spermogones may at once be distinguished by microscopic examination, and by this alone.

8. The spermogones of lichens are now generally regarded as male organs of reproduction—the spermatia being supposed to be analogous in function to the antherozoids or spermatozoids of other cryptogams, and to be endowed with a fecundating or fertilizing influence on the spores. But it is necessary to state distinctly that this is a mere hypothesis, for direct or distinct proof is still wanting. The following circumstances, however, give great support to this

hypothesis, and ought, at all events, to be borne constantly in mind in all speculations on the functions of the spermogones and spermatia:—

a. Intimate relation between the spermogones and apothecia in regard to *site*—the former being generally seated in close proximity to the latter. As illustrations of this intimate relationship, it may be stated that spermogones sometimes occur—
  1. In the hypothecial tissue of the apothecium itself, as in *Celidium fusco-purpureum*, Tul. [Mém., Pl. 14., f. 12.]
  2. On the apothecium itself, as in *Lichina pygmæa* and *confinis*, and sometimes in *Cladonia rangiferina.*
  3. On the exciple of the apothecium, as in *Urceolaria scruposa* and *Parmelia conspersa.*

b. Relative period of development,—the spermogones normally preceding the apothecia. Spermogones should therefore be looked for in specimens bearing no apothecia or young apothecia; when the apothecia are mature, we should expect to find the spermogones old, and perhaps degenerate.

c. Relative abundance of spermatia and spores,—the former being infinitely more numerous than the latter.

d. Relative size of spermatia and spores,—the former being infinitely the smaller.

e. Essential difference in structure between spermatia and spores, the former being solid, simple, or homogeneous, without septa; the latter vesicular, frequently compound or septate, with heterogeneous contents.

f. Essential difference in form, the spermatia being usually elongated and of extreme tenuity, the spores being generally oval or spherical.

g. Greater constancy of size in spermatia than in spores, which are frequently very variable in this respect. Some spermatia are much larger when attached to their sterigmata than when free—as in some *Parmelias;* but this is only an apparent anomaly, for it would appear that they normally split into two on being shed from their sterigmata.

h. Absence of all germinative faculty,—so far as yet known.

i. Similarity in structure between the capsule or envelope of the spermogone and the exciple of the apothecium,—the cellular tissue of which they are composed being generally the same.

k. Constancy of occurrence of spermogones in all lichens, and from every part of the world yet visited by man.

9. Many lichens possess, in addition to apothecia and spermogones, minute organs, outwardly resembling spermogones, called *Pycnides.* They may be described in general terms as papillæform or wart-like bodies, generally black,—sometimes brown,—usually very minute,—generally partially immersed in the thallus, on the surface

of which they are scattered,—occupying a site similar to that of the spermogones. Indeed they differ essentially from spermogones only in containing corpuscles, called *Stylospores*, which are spore-like bodies,—sometimes septate,—usually oval or pyriform in shape,—but varying greatly both in form and size, with oily, distinct contents, always borne on the apices only of simple or unicellular, strongish sterigmata.

10. As the essential difference between spermogones and pycnides lies in the characters of the contained corpuscles, and as their characters are of great importance, as bearing on the physiological functions of the said corpuscles, the comparison or contrast undernoted is made:—

Spermatia. [representing Spermogones.]	*Stylospores.* [representing Pycnides.]
1. Structure solid, homogeneous.	1. Hollow: contents heterogeneous—in part, at least, oily.
2. Always colourless.	2. Sometimes pale yellow.
3. Of extreme tenuity: generally linear—of equal thickness throughout—straight or curved—never spherical.	3. Vesicular: usually oval or pyriform.
4. Of uniform size and shape.	4. Varying more or less in size and shape.
5. Exist in myriads.	5. Greatly less abundant.
6. Absence of germinative power.	6. Presence of germinative power.
7. No oil globules intermixed: imbedded in a mucilage.	7. Oil globules generally intermixed.
8. Borne on the apices and sides of the sterigmata—on the apices when they are simple, on the apices and sides when articulated.	8. Borne on the apices only of sterigmata, which are always simple and stoutish.

11. There is much more dubiety and difficulty regarding the nature of the pycnides and the functions of the stylospores than regarding the nature and functions of the spermogones and spermatia. Two hypotheses have been advanced regarding the pycnides and stylospores, viz.—

a. That they are substitutes for the spermogones and spermatia,—or in other words, a different form or type thereof. What lends probability to this view, is the perfect resemblance to spermogones in outward appearance, site, &c. of such pycnides as those of *Peltigera*, in which the contained corpuscles are the only reason for regarding the conceptacles as pycnides rather than spermogones. But in certain lichens pycnides co-exist with spermogones of the ordinary character.

b. That they are sporoid in function as well as form and general aspect. The strong argument in favour of this view is their germinative faculty. According to this view, pycnides may be regarded as secondary apothecia, or female organs,—and stylospores as secondary or supplementary spores. It has been noticed that, in certain cases, there is a general resemblance in form between the stylospores and the spores of the same species; but this, if not exceptional, is still far from being the general rule.

12. Pycnides are much more common among the lower, or crustaceous, lichens, than among the higher or fruticulose, filamentous, and foliaceous ones; while, on the other hand, the spermogones are most abundant and distinct in the latter. Pycnides would appear to be a connecting link between the lichens and the fungi, and we find them most frequent, perhaps, in species most nearly allied to the fungi.

13. Pycnides may occur in plants or specimens bearing apothecia, but not spermogones; or they may occur alone, both apothecia and spermogones being absent; or pycnides and spermogones, one or both, may occur in species seldom or never bearing apothecia, as in some species of *Strigula*.

14. Pycnides, from their outward resemblances, are apt to be confounded with, or mistaken for,—

a. Spermogones.
b. Minute *Verrucarias*.
c. Minute parasitic fungi, for which, indeed, they have hitherto been almost universally mistaken. Pycnides resemble the fructifications of fungi, called by the older mycologists, *Diplodia*, *Phoma*, *Septoria*, *Cytispora*, *Sclerotium*, *Mesalmia*, *Phyllosticta*, and *Polystigma*.

15. A few lichens,—especially crustaceous ones,—possess several forms of spermogones, or of pycnides, or of both,—though such a phenomenon is comparatively rare. This is just what occurs in such fungi as *Erysiphe*, which has *five* several forms of reproductive organs. The simultaneous occurrence of spermogones and pycnides also characterises certain fungi, *e.g.*, *Sphæria epicymatia*, Wallr. Indeed, the presence of multiform reproductive organs in lichens, renders more indefinite and unsatisfactory the line of definition between them and the fungi; or in other words, renders more clear and intimate the alliance between these two great cryptogamic families.

16. By the discovery in Germany in 1850, and the subsequent description by various French and German authors, of the spermogones and pycnides of lichens, this family of plants has been placed on, at least, as good a footing, in respect of their anatomy and physiology, as other cryptogamic families, which have hitherto been more carefully and successfully studied.

Monday, 21st March 1859.—PROFESSOR KELLAND, V.P., in the Chair.

The following Communications were read:—

1. On some First Principles of a Mental Science, deduced from Correlations of the Primary Laws of Matter and Mind. By Dr Laycock.

2. Verbal Notice respecting the Remains of a Seal found at Portobello. By Dr Allman.

Professor Allman called attention to some bones discovered by Dr Andrew Balfour in a clay field near Portobello, and forwarded by him for presentation to the Museum of Natural History. They prove to be bones of a seal, and consist of some vertebræ, a portion of a scapula, a radius, a femur, and a fibula. They thus afford an additional instance to the few already recorded, of the occurrence of phocine remains in the British Islands. The deposit in which they occurred appears to belong to the period of the boulder clay. They were found about 20 feet above the present level of highwater, and about 15 feet below the surface of the soil.

3. On the Composition of Old Scotch Glass. By Mr Thomas Bloxam, Assistant Chemist to the Industrial Museum. Communicated, with a Preliminary Note, by Professor George Wilson.

We have recently been engaged in the laboratory of the Industrial Museum in executing some analyses of glass, which, as of general interest, I lay before the Society. The analyses in question have been made by Mr Bloxam, the official laboratory-assistant, and it is desirable first to mention with what object they were undertaken. I have limited the investigation in the first place to common bottle glass and window glass in use in Scotland, and so far as could be ascertained, also manufactured in Scotland. As yet we have only overtaken six varieties, and the full import of their analyses will not appear till additional examples have been examined. The following statement aims at nothing more than the establishment of certain data in the glass-manufacture:—

In the case of window-glass, I was anxious to obtain a specimen manufactured before the introduction of the process now so largely followed for the conversion of common salt into soda-ash, the form of alkali now used by the maker of window-glass. Through the assistance of my friend, Mr James Young, I obtained such a specimen. It constituted part of a window-pane, known to have been procured from the Dumbartonshire Glass-Works, and originally employed in glazing a house in Russell Street, Glasgow, built some forty years ago.

Mr John Young, who died last December, aged eighty-five, remembered the building of the house, and knew the source of the glass. He superintended the cutting out of the glass, which was taken from an upper pane, as less likely than a lower one to have been introduced to replace a broken sheet of earlier date. He could also testify that the putty was that originally employed in fixing the pane.

From the analysis it will be seen, that unlike our later window-glass, it contains potash as well as soda. In all probability the alkali used in its manufacture was kelp, which contains both alkalies, and was the chief source of soda to the Scotch glass-maker and soap-maker, till the salt process was established. The glass is inferior in the important quality of colourlessness to the window-glass now manufactured, a defect not explicable by a reference to the presence of potash, which has a less colouring influence on glass than soda. The green tint so manifest is sufficiently explained, however, by the large amount of oxide of iron discovered on analysis. Of dark bottle-glass, specimens abundantly authentic were obtained from the relics of Dr Joseph Black's apparatus. They were probably made at Leith, where ordinary bottles have been manufactured for a long period from clay, sand, salt, and other cheap materials of the neighbourhood. Newcastle, however, has long directed much of the bottle-making to herself, and still more recently Belgium is injuring the trade at Newcastle. In contrast with this I have obtained examples of Chance's glass, made by simply melting ragstone or basalt, the revival of an old French mode of glass-making, extended by the English manufacturer to the production of glass tiles, vases, and large slabs. Could our native trap-rocks be at once melted into available glass, it would be an important addition to the industrial manufactures of the country; but the relinquishment at Birmingham of the basalt process, as unremunerative and impracticable, is not encouraging.

The window-glass from Dunfermline Abbey was given by Dr John A. Smith, a zealous member of the Society of Antiquaries. Dr Smith writes in reference to it—" It was picked up by Bailie Mitchell of Dunfermline, in November 1818, when various diggings were made above the ruins of the old Abbey, preparatory to the rebuilding of the church, and it was said to have been found closely adjoining the site of the great east window of the Abbey.

" Bailie Mitchell gave it to an antiquarian acquaintance of mine, from whom I received it."

The exact age of the glass is thus unknown, and the place of its manufacture quite uncertain. It is, however, certainly old and historically interesting.

Regarding the Abbey, Mr John Stuart, Secretary of the Society of Antiquaries, furnishes the following information:—

" Queen Margaret founded a church at Dunfermline immediately after her marriage in 1070. It was probably of temporary description; and a church completed by David I. was dedicated in 1150."

At the translation of Queen Margaret's relics in 1250, a new church is spoken of, which seems to have consisted in an enlargement of the choir of the previous church, so that the date of foundation and addition would be 1150 and 1250."

The glass from Dunfermline Cathedral was given me by the sexton, as having fallen from the windows, and had been long in his possession.—G. W.

The specimens of glass submitted to investigation were six in number, namely, four window-glasses, one bottle-glass, and one basalt-glass.

1. Window-glass from Dumbartonshire.
2. Fragments from a window in Dunfermline Abbey.
3 and 4. Fragments of window-glass from Dunblane Cathedral.
5. A bottle from the laboratory of Dr Joseph Black.
6. Basalt-glass, manufactured by the Messrs Chance and Co.

Where the quantity of material was sufficient, the following points were determined with each specimen.

1° Specific gravity.
2° Solubility in water.
3° Solubility in alkalies.
4° Solubility in acids.
5° Quantitative composition.

The method of examination was that employed by most chemists for the analysis of silicates, namely, fusing the substance with carbonate of potash and soda, dissolving the fused mass in acid, and estimating the substances in solution. The solvent action of water, acids, and alkalies was ascertained by subjecting the specimen, in fine powder, to each of these menstrua, for some days, and in most cases analysing the liquid containing the soluble part of the glass, The specific gravity was determined in the usual manner practised in the laboratory, a mean number being deduced from the results of three experiments.

Dumbartonshire window glass had a specific gravity of 2·55.

Water dissolved substances from it to the amount of ·14 of a grain per cent., consisting mainly of silica, together with lime, iron, alumina, magnesia, and soda in smaller quantities.

Potash dissolved the glass to the extent of ·76 of a grain per cent.

The quantitative composition of Dumbartonshire glass is the following:—

Silica, . . .	65·51
Protoxide of Iron, .	3·68
Alumina, . .	3·67
Soda, . . .	10·75
Potash, . .	5·55
Lime, . .	10·42
	99·58

Ratio of oxygen in the bases to that in the silica as 3 to 11.

The specimen of Dumfermline Abbey glass was too small to allow of any characters beyond the specific gravity and chemical composition being ascertained. The specific gravity was found to be 2·63; chemical composition to be,—

Silica, . . .	44·47
Protoxide of Iron, .	4·98
Alumina, . .	12·21
Lime, . . .	17·23
Magnesia, . .	3·81
Soda, . . .	13·90
Potash, . .	3·20
	99·80

Ratio of the oxygen in the bases to that of the silicic acid as 3 to 4.

The third and fourth specimens examined were from Dunblane Cathedral windows.

They were of a blue colour, the first being the darker in tint, and were too small to admit of complete examination.

The first sample had a specific gravity of 2·63, and consisted of,

Silica, . . .	62·70
Protoxide of Iron, .	1·26
Alumina, . .	2·60
Lime, . . .	6·25
Oxide ~~Copper~~, .	4·99
Potash, . .	·80
Soda, . . .	21·40
	100·00

Ratio of oxygen in bases to that of the silicic acid as 3 to 10.

The second specimen had a specific gravity of 2·47, and chemical composition as follows:—

Silica, . . .	49·66
Protoxide Iron, .	6·60
Alumina, . .	11·52
Lime, . . .	19·25
Oxide of ~~Copper~~, .	1·32
Potash, . .	1·53
Soda, . . .	10·09
	99·97

Ratio of oxygen in bases to that of silicic acid as 3 to 5.

The next specimens submitted to investigation, was a bottle used by Professor Black for chemical purposes.

The specific gravity was 2·75.

Water was found to dissolve from it ·22 of a grain per cent., consisting of lime, oxide of iron, silica, and magnesia.

Hydrochloric acid dissolved substances to the amount 2·82 per cent., and potash, ·56 of a grain per cent.

The glass had the following composition:—

Silica,	56·90	Ratio of oxygen in bases to that of silicic acid as 1 to 2.
Protoxide of Iron,	3·61	
Alumina,	10·22	
Lime,	25·80	
Potash,	1·90	
Soda,	1·55	
	99·98	

The last sample made the subject of analysis was the so called basalt glass of Messrs Chance and Co.

It is produced by the mere fusion of basalt, is very hard, and of a black colour.

The specific gravity was ascertained to be 2·709.

Water dissolved ·34 of a grain per cent., consisting of silica, protoxide iron, lime, and magnesia.

Hydrochloric acid dissolved 3·4 per cent., and potash 1·44 per cent.

The composition of the basalt glass may be thus represented:—

Silica,	54·88	Ratio of oxygen in bases to that of silicic acid as 1 to 2.
Protoxide Iron,	17·83	
Alumina,	11·54	
Lime,	9·26	
Potash,	1·05	
Soda,	5·34	
	99·90	

Action of Solvents upon the foregoing varieties of Glass.

		Action of Water.	Action of Hydrochloric Acid.	Action of Potash.
I.	Dumbartonshire glass, ...	Dissolved ·14 of a grain per cent.	Dissolved ·72 of a grain per cent.	Dissolved ·76 of a grain per cent.
II.	Dunfermline Abbey glass, . .	Too small for experiment.	... ...	... ...
III. IV.	Dunblane glasses, ...	Do. Do.	... ...	... ...
V.	Black's glass, ...	Dissolved ·22 of a grain per cent.	Dissolved 2·82 grains per cent.	Dissolved ·56 of a grain per cent.
VI.	Basalt glass, ...	Dissolved ·34 of a grain per cent.	Dissolved 3·4 grains per cent.	Dissolved 1·44 of a grain per cent.

Chemical Composition.

	I.	II.	III.	IV.	V.	VI.	Specific gravity.
Silica, ...	65·51	44·47	62·70	49·66	56·90	54·88	I. 2· 55
Protoxide Iron, ...	3·68	4·98	1·26	6·60	3·61	17·83	II. 2· 63
Alumina, ...	3·67	12·21	2·60	11·52	10·22	11·54	III. 2· 63
Lime, ...	10·42	17·23	6·25	19·25	25·80	9·26	IV. 2· 74
Magnesia.	...	3·81	...	...	...	...	V. 2· 75
Oxide Cobalt, ...	...	...	4·99	1·32	...	...	VI. 2·709
Potash, ...	5·55	3·20	·80	1·53	1·90	1·05	. .
Soda, ...	10·75	13·90	21·40	10·09	1·55	5·34	...
	99·58	99·80	100·00	99·97	99·98	99·90	
Ratio of Oxygen in bases to that of Silica,	3:11	3:4	3:10	3:5	1:2	1:2	

Monday, 4*th April* 1859.—PROFESSOR CHRISTISON, V.P., in the Chair.

The following Communications were read:—

1. On the Gradual production of Luminous Impressions on the Eye. Part II. By Professor Swan.

The author, in a paper communicated to the Royal Society of Edinburgh in 1849,* described a method of observation by which he had succeeded in measuring the brightness of visual impressions of short duration. This consisted in causing a disc, with a sector of a known angle cut in it, to revolve with a known uniform velocity between the eye and a luminous object. At each revolution of the disc a flash is seen. The time during which the light has acted on the eye is easily computed from the angle of the sector and velocity of the disc; and the brightness of the flash is ascertained by photometric arrangements. By this method the brightness of impressions formed on the eye by light acting for short intervals of time varying from ·1 to ·001 of a second was ascertained with results which have been described in the paper already referred to.

It seemed desirable to extend the observations to impressions formed on the eye in intervals of time still shorter than ·001 of a second; and it may seem that this could be accomplished, either by diminishing the angle of the sector, or by increasing the diameter or velocity of rotation of the disc. There are obviously, however, limits to the narrowness of the sector, and to the diameter of such discs as can be used conveniently; and the velocity with which the disc may be driven is also limited, for when the number of revolutions exceeds about ten in a second, the successive impressions, which it is proposed to observe separately, become blended into a single nearly uniform impression, owing to their persistence on the retina. The instrument now described to the Society is devised for the purpose of separating a *single* impression out of the multitude of impressions made by a rapidly revolving disc, so as to render it possible to observe the brightness of isolated visual impressions formed by light acting on the eye for extremely short intervals of time.

The instrument consists of a train of wheels and pinions by which a disc having a sector cut in it is driven with great velocity. The numbers of teeth in the wheels and pinions are so arranged that each wheel, as well as the disc, makes ten revolutions for one revolution of the wheel by which it is driven. Each of the two last wheels of the train, which are of solid metal, has a hole pierced in it, through which light transmitted by the sector can pass to the eye; and the wheels are so placed that at each hundredth revolution of the sector, and only then, the sector in the disc and the holes in the wheels come into the same straight line, so that the eye of the observer receives a single flash transmitted through the holes in the wheels. The result of this arrangement is, that although the disc be driven at the rate of a hundred revolutions per second, so that the impressions produced by the successive flashes transmitted by it when seen by the unassisted eye would be blended into an uniform impression, yet the observer, looking through the holes in the wheels, receives only a single flash of light once a second. The brightness of the observed isolated flashes may be ascertained by photometrical means, similar to those employed by the author for the same purpose in 1849, and which he has fully described.

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* Edinburgh Transactions, vol. xvi., p. 581.

An Instrument for producing Isolated Luminous Impressions of extremely short duration, varying from one-tenth to one millionth of a second, was shown.

2. On the Destructive Effects of the Waves of the Sea on the North-East Shores of Shetland. By Thomas Stevenson, C.E., F.R.S.E.

The author stated, that the present communication might be regarded as supplementary to the one describing the results of his marine dynamometer, which would be found in the 14th volume of the "Transactions." On the Bound Skerry of Whalsey, which is only exposed to the waves of the North Sea or German Ocean, he had found, on first landing in 1852, masses of rock, weighing 9½ tons and under, heaped together by the action of the waves at the level of no less than 62 feet above the sea; and others, ranging from 6 to 13½ tons, were found to have been quarried out of their positions *in situ*, at levels of from 70 to 74 feet above the sea. Another block of 7$\frac{7}{10}$th tons, at the level of 20 feet above the sea, had been quarried out and transported to a distance of 73 feet from S.S.E. to N.N.W. over opposing abrupt faces as much as 7 feet in height. Somewhat similar evidences of the force of the sea were observed on the neighbouring islands, and more recently by Mr David Stevenson at Balta and Lambaness (in the most northern of the Shetland Islands), who, in a report made at the time, attributed the great force of the waves in the northern regions of the German Ocean to their exposure and the proximity of deep water to the land. In addition to these causes, the author referred to the strength of the tides, the configuration of the German Ocean, and to the great general depth of the water, as the probable causes why heavier waves are produced in the latitude of Shetland than are found, for example, on the coasts of England or Holland. The author, after alluding to the writings of Mr Airy and Mr Webster, referred, in particular, to this gradually decreasing general depth in passing from Shetland to Holland, as a main cause of the diminished magnitude of the undulations. That the waves are materially smaller in the southern than in the northern latitudes, may be inferred from the low, yet safe, level at which many of our southern sea-port towns have been built in reference to that of high water. The author considered that another proof that this reduction of the waves depended on the reduction in the depth of water, might be deduced from the structure of the bottom. He considered that the presence of *mud* at any depth might be taken as a certain proof that the agitation, originating at the surface, had ceased to be appreciable. If the geological formation did not produce a clayey deposit, or if strong submarine currents existed, the *absence* of mud might afford no proof of the magnitude of the waves; but its *presence* in shoal water may be relied on as indicating with certainty that in whatever locality it is found there must be small disturbance at the surface, or, in other words, that there cannot be a heavy sea. Applying such a *test* to the present case—muddy deposits are found in from 80 to 90 fathoms off Whalsey, from which point southwards they are found in gradually lessening depths, till they rise to within 8 fathoms of the surface at the mouth of the Elbe. Similarly, at the Firth of Forth, the mud rises on the north side from 18 fathoms off Elie Ness to 7 at Burntisland; and on the south side, from 17 fathoms, near North Berwick, to 2 fathoms off Leith; while above Queensferry, even although the current is stronger in the higher portions of the estuary, the mud, owing to the comparative absence of waves, actually emerges above low water. It is well known, that on the banks of Newfoundland, and all round the British islands, where the bottom suddenly rises near the 100 fathoms line, the waves actually break. It seems reasonable, therefore, to infer, that the gradually decreasing depth of the German Ocean must as effectually, though not so suddenly, diminish the size of the undulations.

3. Notice of an Unusual Fall of Rain in the Lake District, in January 1859. By John Davy, M.D., F.R.SS. Lond. and Edin.

Whilst the average fall of rain during the preceding six years in January has been at Ambleside 4·22 inches; in this month, in the current year, the rain measured has amounted to 14·82 inches.

The quantity of rain that has fallen in other localities of the district during the same month is stated in a table, in which also is included the rain-fall in some other parts of the United Kingdom. In the former, it has ranged from 14·375 to 6·514 inches, diminishing with distance from the central mountains; in the latter, the range has been from 6·48 inches to 0·36 inch, diminishing, it would seem, with distance from the western coasts.

In another table some other instances of extraordinary rain-falls are given which occurred in the Lake District at Coniston and Ambleside, varying from 12 to 24·39 inches in a month.

A third table is appended, showing the quantity of rain monthly that has fallen at Seathwaite in Borrowdale during fourteen years; from which it appears that the maximum fall monthly has been 32·83 inches, and yearly 160·55 inches.

In connection with the rain-fall, other meteorological observations are offered, illustrative of the peculiarities of the last year and of the present season; of which, as regards the last, the most remarkable are, a prevalency of westerly winds, exceeding mildness, and a precocious spring; vegetation in the first week in March being at least a month in advance.

Royal Physical Society.

Wednesday, 23d February.—Andrew Murray, Esq., President, in the Chair.

The following Communications were read:—

I. *Description of some Fossil Bovine Remains found in Britain.* By Wm. Turner, Esq., M.B. Published in this Number of Journal, p. 31.

II. *On* Goodsirea mirabilis, *an undescribed Gymnothalmatous Medusa.* By T. Strethill Wright, M.D., &c. This Communication will appear in a future Number of the Journal.

III. *Notice of some Birds observed in the Island of Heligoland, in a Letter from W. H. Gatke, Esq.* Communicated by Professor Balfour. (Specimens were exhibited.) We give the following extract from this communication:—

"I have ventured upon sending a few birds obtained on this island (Heligoland), which probably may prove acceptable to your College Museum. They are a female of a species labelled in the College Museum, if I recollect right; *Sylvia Tytleri*, obtained somewhere in India; it is the *Muscicapa parva* of continental ornithologists, a bird visiting this island almost every autumn, and very early in spring. The specimen in the College Museum is an old male bird,—the one I send an old female. To my great regret, I have not been able to obtain a more perfect sample of this pretty little flycatcher." [The specific term of *Rubecola Tytleri* was given to the bird referred to above by the late Professor Jameson, when he exhibited it at a meeting of the Wernerian Natural History Society in April 1835. He considered that in the form of the bill it presented as it were a link between the genera *Rubecola* and *Phœnicura*. The bird was sent to the University Museum by Lieutenant Tytler from the Himalayan Mountains. It is also a bird of eastern Europe; and is very rarely seen in collections. The sexes are described by Gould in his "Birds of Europe" as being similar in colour. This specimen resembles that figured by Gould as a young male.] "The rest of the birds contained in the box belong to *Sylvia cærulicula* of Pallas

(I. p. 480). I have furnished a sufficient number to exemplify all changes of plumage occasioned by age, season, and sex. *This form* of the blue-throated warbler frequents Heligoland rather abundantly every autumn; in spring its visits depend much on the weather; a warm and wet May brings great numbers of the finest adult birds, whereas with dry cold weather only some solitary individuals make their appearance,—unfortunately the month of May is generally cold and dry in this island. The common blue-throated warbler, with the *white* spot in the blue (Yarrell, I. p. 254), is here a very rare bird, scarcely *one* specimen turning up in every five years, in spite of its being quite a common species on the adjacent coast of North Germany." [With the *white* spot it is the *Phœnicura suecica* of British naturalists, which is believed to become *red* with age, as in some of the specimens exhibited; and is a rare bird in England, only a very few instances of its occurrence being on record.] "I have added these few observations, as they perhaps may interest your ornithological friends; for the same purpose I have inclosed a catalogue of the birds that have, according to my observations, visited this island during the last fifteen years." [This list was published in the last Number of this Journal, p. 334.] "If the small area of this place (scarcely half a square mile English) is borne in mind, the number of its feathered visitors will be admitted to be wonderful. As this catalogue (including some 320 species) contains the names of several specimens *hitherto not known as European*, it will, I trust, prove welcome to all who take an interest in the ornithology of our part of the globe."

IV. *On the Danger of Hasty Generalization in Geology.* By Alexander Bryson, Esq.

V. *Notes on some points in the Natural History of the West Coast of Ross-shire.* By John Alex. Stewart, Esq., Lochcarron.

Wednesday, 23d March.—William Rhind, Esq., President, in the Chair.

The following Communications were read:—

I. *Extract of Letter from the Rev. Hugh Goldie, Old Calabar, to Dr Greville, respecting some singular silk "Bags" formed by Insects in Africa.* Communicated by W. H. Lowe, M.D.

II. *On some recently-discovered Eyeless Beetles from the Caves of Carniola and Hungary.* By Andrew Murray, Esq.

Mr Murray exhibited a fine series of eyeless beetles from the various caverns, &c., where these curious and rare animals have been found. There were twenty-six different species shown, of which there were a number which had been only discovered and described within the last two years, the possession of which he owed to his friend Herr Dohrn of Stettin. He pointed to the two new genera—Pholeuon and Drimeotus—as being of special interest, as filling up a blank between the genera Leptoderus and Adelops, and proving that the former of these genera truly belonged to the family of the Cholevidæ, instead of being allied to the genus Mastigus, as was supposed by Lacordaire and other authors.

III. *Notice of the Tenacity of Life in* Buccinum coronatum. By ALEXANDER BRYSON, Esq.

IV. *Observations on British Zoophytes.* By T. STRETHILL WRIGHT, M.D. This Communication will appear in a future Number of Journal.

V. (1.) *On the Vomer in Man and the Mammalia, and on the Sphenoidal Spongy Bones.* (2.) *On the Vertebral Columns of two Deformed Codlings.* By JOHN CLELAND, M.D.

VI. *On the Discovery of Nullipores* (*Calcareous plants*) *and Sponges in the Boulder Clay of Caithness.* By CHARLES W. PEACH, Esq., Wick. (Specimens were exhibited.)

Wednesday, 27th April 1859.—T. STRETHILL WRIGHT, M.D., President, in the Chair.

I. *Report on the Pearl Banks of Aripo, Ceylon, for Season* 1858. By E. F. KELAART, M.D. Communicated by Dr R. K. GREVILLE.

In this paper, the author states that he found in most of the pearl-bearing shells a worm (a species of *Filaria*), which he considered had much to do with the formation of pearls. This worm he found in large numbers in the liver, ovary, mantle, and other parts of the oyster. He also considered, from his researches into the subject, that the ova of the oysters and the ova of worms form the nuclei of many pearls found in the soft parts of the animal, according to the doctrine of Sir Everard Home.

II. *Note on the Lantern Fly of Honduras.* By Mr JAMES BANKS. Communicated by Dr J. A. SMITH.

At a previous meeting, Mr Banks exhibited a specimen of the lantern fly (*Fulgora laternaria*) of Honduras, and doubts having been cast on Madame Merian's statements as to its being really luminous at times or not, Mr Banks was requested to get farther information if possible on the subject. He had received letters from correspondents in Belize, and they bore testimony to the truth of the statement that this fly really emits a light. Mr Alexander Henderson, Belize, says:—"The fly certainly possesses light, and therefore emits it. The light is evidently under its control, for it increases and diminishes it at pleasure. When the wings are closed, there are three luminous spots, one on each side of the head part, giving out a beautiful sulphur-coloured light, in rays that spread over the room. The third luminous spot is seen when the fly is on its back, half-way down the abdominal part of the insect. When quiescent, the lumination is least; in daylight the upper spots are nearly white, emitting no light whatever (its lively time is at twilight). Immediately on being agitated, or moving about, the spots become sulphur-coloured, and radiate forth streams of light, clearly seen although the sun be shining into the room, as it now does at the moment I write, with the creature in the glass tumbler before me."

III. (1.) *Notice of a Fossil Nautilus from the Isle of Sheppy.* By JAMES M'BAIN, M.D., R.N.

Dr M'Bain said, that the specimen of a fossil nautilus, which he exhibited to the Society, was obtained from the Isle of Sheppy, along with other fossil remains, and presented to him by his friend Dr Easton of H.M.S. Pembroke. Before placing the specimen in the public museum of Edinburgh, which, under the present energetic management, was rapidly assuming a highly scientific character, he considered it advisable to give a short description of the state of preservation of the shell, and the locality whence it was derived. The species agreed with the description of the *Nautilus Sowerbyi*, given in a monograph of the Eocene Mollusca, by F. E. Edwards, and published by the Palæontographical Society. The specimen he exhibited was seven inches in diameter by four inches across, measured from one umbilical space to the other. It had lost the whole of the outer or inhabited chamber, but the triangular shape of the aperture was distinctly shown by the compressed ventral margin of the remaining part of the shell. The position of the siphuncle —said to be close to the dorsal margin—seemed to be indicated by a small deposit of iron pyrites, probably caused by the organic matter having been longer retained in that canal. The interior of the shell was filled with a ferruginous clay, containing a large proportion of carbonate of lime, a stalagmitic layer of which was seen encrusting a portion of the mass. Defoliation of the outer porcellanous coat of the shell had taken place, but what remained of the inner nacreous layer showed the striæ of growth, bending sharply backwards in well-marked undulations. If the proportions of the fossil specimen were similar to those of the recent *Nautilus pompilius*, the shell, when entire, would have measured about fifteen inches in diameter.

(2.) *Notice of the* Nucula decussata, *found in the so-called Raised Sea-beach Bed at Leith.* By James M'Bain, M.D., R.N.

IV. *Contribution to a Monograph of Iceland Spar.* By Alexander Bryson, Esq.

V. *On a Method of Constructing Singly-refracting Polarizing Prisms of Nitrate of Potash.* By T. Strethill Wright, M.D.

VI. *Notes on the Geology of Swellendam, South Africa.* By William Carruthers, Esq. (Specimens of the rocks and fossils were exhibited.)

The specimens which formed the subject of this notice were gathered and sent to this country by Mr Robert Douglas, a self-educated and enthusiastic geologist. The collection consists of nearly 1000 different specimens, with accompanying manuscripts, drawings, and sections, most elaborately executed. Valuable memoirs had been published by Mr Bain on the geology of South Africa, but he did not seem to have visited Du Toit, the locality where the present fossils had been gathered. Du Toit is about thirty-five miles north-west from Swellendam. It is situated on the further side of the Lange Bergen Mountains, on the oldest beds of what, from palæontographical evidence, seems to be the Old Red Sandstone. The fossiliferous beds are composed of coarse sandstones and shales. The fossils are chiefly brachiopod Mollusca, and include *Spirifer antarcticus*, and *Orbignii*, *Terebratula Bainii*, *Orbicula Bainii*, &c., *Solinella antiqua*, and other bivalves. Besides these, there were only some Encrinite stems, and the cast of a Trilobite, neither of which could be more specifically described, owing to their bad preservation.

VII. *Ornithological Notes.* (Specimens were exhibited.) (1.) *On the Gadwell,* Anas strepera, Linn. By P. A. DASSAUVILLE, Esq.

Specimens of this bird were shown, which had been shot on the Tay, near Newburgh, in the beginning of April.

(2.) *On the Shoveller,* Anas clypeata, Linn.; *the Great Grey Shrike,* Lanius excubitor, Linn.; *and the Shore Lark,* Alauda alpestris, Linn., shot at Tynefield, near Dunbar. By JOHN ALEXANDER SMITH, M.D.

VIII. *Notes on the Crania of* Bos primigenius *in the Museum of the Society of Antiquaries of Scotland.* By JOHN ALEXANDER SMITH, M.D.

The crania of the *Bos primigenius,* in the Museum of the Society of Antiquaries, were, Dr Smith believed, perhaps the earliest specimens recorded as found in this country. The first was sent by the Rev. Thomas Robertson, minister of Selkirk, as far back as 1781. In a letter sent with the skull, he states that he saw five of the same kind in a marl moss at Whitemuirhall, Selkirkshire (where this specimen was obtained), and along with them were several small axes, resembling those used by coppersmiths.

The Botanical Society.

Thursday 10*th February.*—ANDREW MURRAY, Esq., President, in the Chair.

Professor Balfour stated that he had received from Dr Lindley specimens of the seeds of a hybrid between the Pea and Lentil. This hybrid has been produced by Dr Rauch of Bamberry. It bears long pods, and is very productive. The taste of its seeds is very agreeable. The colour of the seeds is like that of the best ripened early frame pea, a little heightened so as to verge upon apricot. In form the seeds are compressed like a lentil, not spherical like a pea. In size they are generally about twice as large as that of a fine lentil, or even somewhat bigger, but they are by no means alike in magnitude. (Specimens of the seeds were exhibited.)

The following communications were read:—

1. *On Approximate Measurements of the Axial Appendages of Plants.* By WILLIAM MITCHELL.

Any one who takes an interest in botanical studies, if at the same time of a mathematical turn, can scarcely fail to wish for some method, the application of which would give analytical expressions for those laws which determine the symmetric forms, the graceful curves, and varied contour of leaf and flower and fruit, so conspicuously displayed throughout the wide range of the vegetable kingdom. Unhappily, however, the more closely the subject is considered, the more hopeless does the realization of this wish appear; for amidst curves of double curvature, often altering their flexure under every change of temperature, or bending beneath the slightest touch, amidst curvilineal angles and doubtfully defined points, our nicest mensuration seems utterly at fault.

Yet plants must have a geometry, though we should never be able to reach its transcendental heights nor expound its intricate formulas. But though foiled for the present in this direction, might not something be attempted to obtain derivative laws, so as to exhibit the relations of approximate values? The importance of mean results in meteorology, astronomy, statistics, and other departments of science, is well known; and could we introduce them into botany with equal advantage, a desirable end might be gained.

The idea has often presented itself to my mind while rambling among the blooming beauties of the vegetable world, during intervals of leisure; but after various expedients with subsequent trials and failures, I have got no further than a rough method of measurement of the axial appendages of plants, which this paper is intended to explain, merely as a tentative process which may to a limited extent be ultimately found useful.

There are many leaves, sepals, and petals of plants, which may be gently pressed on a plane surface, without very greatly disturbing the natural form, and an accurate outline traced. Having obtained an outline by this, or any other way, I next choose a convenient point within it, and after certain measurements about to be described, find an equation of the form,—

$$\begin{aligned} R = A &+ B \sin(\theta + C) \\ &+ B' \sin(2\theta + C') \\ &+ B'' \sin(3\theta + C'') \ \&c.; \end{aligned} \qquad (1.)$$

which is an adaptation to our present purpose of a formula often employed by meteorologists, and which will be found well explained in the article Meteorology in the 8th edition of the "Encyclopædia Britannica." From the point selected as the origin of measurement I draw radii vectores to the outline, corresponding to equal arcs, into which a circle described round the pole or point in question is divided. On each side of each of these primary radii, other radii are drawn in the same way, and at equal distances, and each measured by a scale of equal parts. Then the lengths of each principal radius, added to that of each of the secondary drawn on each side of it, and the sum divided by the number of the whole, gives a mean radius corresponding to each primary division of the circle. Let the total number of mean radii be called N, and each in succession denoted $r_1, r_2, r_3, \ldots r_n$, the corresponding arcs may be represented by $\theta, 2\theta, 3\theta \ldots n\theta$, and their cosines and sines by $c_1, c_2, c_3 \ldots c_n$; $s_1, s_2, s_3 \ldots s_n$, then by the method of least squares we can easily find the values of the quantities A, B, C, B′, C′, &c., in equation (1.), thus:—

$$A = \frac{1}{N}(r_1 + r_2 + r_3 \ldots r_n),$$

and by taking

$$a_1 = \frac{2}{N}(c_1 r_1 + c_2 r_2 + c_3 r_3 \ldots c_n r_n),$$

$$a_2 = \frac{2}{N}(c_2 r_1 + c_4 r_2 + c_6 r_3 \ . \ . \ . \ c_{2n} r_n), \text{ \&c.}$$

$$b_1 = \frac{2}{N}(s_1 r_1 + s_2 r_2 + s_3 r_3 \ . \ . \ . \ s_n r_n),$$

$$b_2 = \frac{2}{N}(s_2 r_1 + s_4 r_2 + s_6 r_3 \ . \ . \ . \ s_{2n} r_n), \text{ \&c.},$$

we find

$$B = \sqrt{a_1^2 + b_1^2}; \ \tan C = \frac{a_1}{b_1}$$

$$B' = \sqrt{a_2^2 + b_2^2}; \ \tan C' = \frac{a_2}{b_2}, \text{ \&c.}$$

If the origin of measurement be taken on any part of the axis, except the centre of the leaf, sepal, or petal, we shall have simply, when the formula is reduced,—

$$6\,B = \sqrt{3}\,(r_1 - r_5) + (r_2 - r_4) - r_6 + r_3$$
$$6\,B' = r_1 - r_2 - r_4 + r_5. \qquad (2.)$$

for 12 divisions of the circle, or $\theta = 30°$, and C, $C' = 90°$ or $270°$, according as B, B′, come out positive or negative; A will be found as before, namely, $A = \frac{1}{12}(r_1 + r_2 + r_3 \ . \ . \ . \ r_{12})$ in this case.

In this way I have roughly measured a number of leaves, placing the pole in the axis, at $\frac{1}{4}$th the length of the midrib from the base, and using a scale of $\frac{1}{20}$th of an inch; but as each radius was taken only as the mean of three, and the measurement made merely to satisfy myself as to the practicability of the method, a few of the results need only be given here by way of illustration.

Laurel,	$R = 34{\cdot}6 + 18 \sin(\theta + 90°)$
Hollyhock,	$R = 38{\cdot}4 + 24 \sin(\theta + 90\ )$
Strawberry,	$R = 15 \ + 10 \sin(\theta + 90\ )$
Beech,	$R = 29 \ + 15 \sin(\theta + 90\ )$
Ivy,	$R = 40 \ + 16 \sin(\theta + 90\ )$
Common Dock,	$R = 24 \ + \ 8 \sin(\theta + 90\ )$
Common Sorrel,	$R = 21 \ + \ 6 \sin(\theta + 90\ )$

These polar equations are not carried further than the first variable term, but as the series is very convergent, one or two variable terms might be a sufficient approximation in the case of most plants capable of being so measured. The formula will apply to any outline of any part of a plant presenting a closed curve, and involves only a very simple numerical operation.

It might be applied also to such unfavourable cases as those of the leaves of *Plantago lanceolata*, *Leontodon Taraxacum*, and the still more elongated leaves of grasses, &c.; but then the point at the base is too obscure for giving the measure of a proportional part of the axis, in order to compare the resulting formulas with those of other leaves, so readily as in the former cases.

In conclusion, permit me to remark that, if we had an extensive series of accurate measurements, in the way thus indicated, we would be able to compare both the average variation in form of the axial appendages of any plant, and also that of different plants, by which we might meet with numerical relations, tending to throw light on vegetable morphology, and otherwise advance the interests of the attractive science of botany.

2. *Botanical Notes of a Visit to Cannes* (dep. Var.), *France, in the Spring of* 1858. By Mr R. M. Stark.

3. *Report on the Conservation of Forests in India.* By Dr Cleghorn. Communicated by Professor Balfour.

Dr Cleghorn says :—" During the past year I proceeded on my first tour of inspection, traversed Mysore, and visited the depôts at the mouths of nearly all the rivers on the Malabar coast, examining the greater part of the western Ghauts with a view to ascertain the exact state of the Government forests, their extent and capabilities. I travelled through the most wooded portions, along the chain of Ghauts, ascending and descending by the mountain passes, from the Bombay frontier down to Ponany. I afterwards went across the Anamallay Hills, and round the slopes of the Neilgherry Hills ; I also made a circuit of the Wynaad, and twice visited the Conolly plantations at Nellumboor, being altogether eight months absent from the Presidency. In the beginning of the century an immense almost unbroken forest covered the western Ghauts, from near the water's edge to the most elevated ridges, left to nature, thinly peopled, abounding in wild animals, and all the higher portions, without exception, covered with timber ; and now the passing traveller, looking down from the higher peaks of Coorg or Malabar, conceives that an inexhaustible forest lies below him ; but as he descends the Ghauts, he finds that the best timber has been cut away, and that the wood contractor is felling in more remote localities. I speak especially of Teak, Blackwood, and Poon spars, which are every year becoming more scarce in accessible situations. The practice in this country has been the converse of that in Europe, where the soft wood is thinned out and the hard wood left ; here the valuable kinds are removed and the scrub left. By one of these authorities, burning the jungles was recommended as a sanitary measure, and to diminish the number of wild animals ; but circumstances have much changed. Now the axe of the coffee planter and of the coomree cultivator has made extensive and often wanton havoc, devastating a large portion of the area of the primeval forest. The trees are classified according to size—1st class, 6 feet in girth ; 2d class, 4½ feet ; 3d class, 3 feet and upwards ; 4th class, under 3 feet, seedlings.

Teak (*Tectona grandis*). This invaluable wood has received the special attention of the department, and, I may say, has occupied at least two-thirds of my own time during the past year. Along the whole length of the Malabar coast, from Goa to Cochin, there is now very little of this wood in a ripe state on Government land below the Ghauts, and there are only three localities above the Ghauts where I found Teak in abundance, and of good size,—viz., 1st, The Anamallay Forest in Coimbatore ; 2d, Wynaad and Heggadevincottah (partly in dispute between Mysore and Malabar) ; 3d, Goond Tableau, North Canara, near Dandellie. The Anamallay forests have been the subject of annual reports to Government since 1848, when their importance was first declared by

Captain F. C. Cotton, in the Madras Journal. The forests of Wynaad and Heggadevincottah teak, on the borders of Mysore and Malabar, are of great value, and stand second in importance. The average price of teak at the quarterly auctions held at Mysore has been almost exactly the same as at Anamallay, about one rupee per cubic foot. The Canara teak is of much smaller scantling generally than that of Wynaad. It has the advantage of water-carriage to the coast, not possessed by the last two; but it has for some years been chiefly obtained for naval purposes from the banks of the Kalla Nuddee, where it emerges from the Soopah Hills, and the supply has gradually been sent down from more distant localities, as in Malabar, where the teak is now cut by the Teroopaud of Nellumboor just under the Neilgherry peak. The Goond Forest is the chief remaining reserve in Canara. I saw here several thousand trees on an elevated plateau with precipitous sides. The trees are well grown and ripe, conserved, by their inaccessible position, which has been rarely visited by Europeans. Poon spars (*Sterculia fœtida*) are becoming very scarce, and consequently are perhaps more valuable than teak; young ones, especially such as are in accessible places, are most carefully preserved. Blackwood (*Dalbergia sissoides*) is a valuable wood. It has risen much in price. Indents were received during the year, both from Madras and Bombay gun-carriage manufactories, each for 5000 cubic feet. There is not much blackwood remaining in the Anamallay Forest, but there is a considerable quantity in the escheated forest of Chennat Nair, and it is abundant in the Wynaad and Coorg.

Sappan Wood (*Cæsalpinia Sappan*).—This important dyewood has engaged my attention. It appears to grow with great luxuriance in South Malabar, and is cultivated rather extensively by the Moplahs, who plant a number of the seeds at the birth of a daughter. The trees require fourteen or fifteen years to come to maturity, and then become her dowry. I saw more on the banks of the Nellumboor River than anywhere else; why it should be there in particular is not obvious, as Malabar is generally uniform in its character. A better system of cutting and cultivating the sappan is desirable. This dyewood is also damaged. I believe, by being allowed to float in salt water.

The *Sandal Wood* tree (*Santalum album*) in Mysore, Canara, Coimbatore, Salem, and a little in North Arcot, has received much attention. It would appear that the spontaneous growth of this tree has increased to a considerable extent.

The *Gutta Percha* tree of the western coast (*Isonandra sp ?*) has been traced from Coorg to Trevandrum. All the reliable information procurable has been condensed into a memorandum, and a large sample has been transmitted to England for report as to its suitability for telegraphic and other purposes.

Catechu (*Acacia Catechu*).—The enhanced value of cutt has caused an unusual destruction of this tree.

Kino.—Two thousand trees of the kino tree (*Pterocarpus Marsupium*) were seen along the roads through the Wynaad notched for the extraction of Kino, which is taken to the coast, where it meets with a ready market, and is exported in wooden boxes to Bombay.

Bamboos.—Immense quantities of fine bamboos are floated down the rivers of the western coast. It is one of the riches of the provinces.

They are ordinarily 60 feet long, and 5 inches in diameter near the root. These are readily purchased standing at five rupees per 1000, and small ones at three and a half rupees per 1000. Millions are annually cut in the forests, and taken away by water in rafts, or by land in hackeries; from their great buoyancy they are much used for floating the heavier woods as Mutte (*Terminalia tomentosa*), and Biti (*Dalbergia arborea*), and piles of them are lashed to the sides of the Paltimars going to Bombay. The larger ones are selected as out-riggers for ferry-boats, or studding-sail booms for small craft. In addition to the vast export by sea, it is estimated that two lacs are taken from the Soopah talook eastward. The Malabar bamboo is much smaller than that of Pegu (*Bambusa gigantea*), which is 8 inches in diameter.

Mode of Floating Timber.—It is curious to see the clever management of the floaters, who are a distinct class of persons. Rafts are of all sizes, usually longer than broad, and the logs bound together with the stringy bark of various trees, and stout branches passing through the dragholes at right angles to the log. In the centre of the raft a small hut is generally made of thatch, or bamboo laths, covered with Palmyra leaves; in this the floaters are sheltered at night. It is not usually considered advisable to float logs when the river is at the fullest, as the raft is apt to go over the bank and be stranded. Numerous logs may be seen high and dry all along the sides, and the following year the flood lifts them. At night, floats are brought too under steep banks in deep water; they are then tied to the trunk of some adjoining tree; occasionally the banks fall in, and serious accidents occur.

Coffee.—The successful cultivation of the coffee plant is extending remarkably, and applications for grants of forest land pour in upon the revenue authorities. In the Sisipara Permbady, and Sumpagee passes vast clearings are being made. In the Coonoor Ghaut six large plantations may be seen, and there are very large and numerous holdings, above thirty, in the Wynaad, which from year to year will increase. The plant has succeeded admirably in Mysore, and there are patches of cultivation in Madura, and even in North Canara. I may observe that in granting forest land, it seems to me that while the destruction of forest (Teak, Ebony, and Poon spar excepted), for bona fide cultivation, may be considered legitimate, yet the preservation of the fringe along the crest of the mountain ridges is of special importance in a climate point of view, and this should never be given over to the axe. As these mountain crests are not suitable for the growth of coffee, the restriction cannot be complained of.

Tea.—I think it right to bring to the notice of Government the thriving condition of a tea plantation near Coonoor, belonging to Henry Mann, Esq., who has devoted much attention to it, and has spared no expense. This is a very interesting experiment. The best varieties of the shrub were imported from China in 1854, the seeds having been given to Mr Mann by Mr Fortune on his return from the tea-growing districts; there are now about 2000 vigorous plants, and to ensure success, it seems only necessary to procure a supply of workmen to teach the manipulation and separation of the leaves. I have issued a general instruction to the forest assistants in a circular, and I try to persuade each to keep a small arranged herbarium of flowers and fruit-bearing specimens of all forest trees and their varieties, with notes. By inviting them to do this, I trust some will become at least observers, if not botanists. I am preparing a Manual of Indian Botany, which it is hoped may be a useful guide to the botanical riches of the Presidency.

Thursday, 10*th March.*—ANDREW MURRAY, Esq., President, in the Chair.

Professor Balfour exhibited a specimen of *Cephalanthera ensifolia,* collected by Mr A. Buchan, near Comrie, not far from the Devil's Cauldron.

The following Communications were read :—

1. *On the Manner of Growth of* Dracæna Draco *in its natural habitat, as illustrating some disputed points in vegetable physiology.* By Professor C. PIAZZI SMYTH.

After alluding to the vertical theory of growth in plants, as maintained by Petit-Thouars and Gaudichaud, and the horizontal theory of Mirbel and Trecul, together with the last pronunciation of opinion thereon by the French Academy, to which his attention had been called by Professor Balfour, the author proceeded to describe such characteristics as he had been able to make out in the Dragon trees of Teneriffe, growing there indigenously, and through various periods of time, from five to, as it has been alleged, in the case of one specimen, five thousand years; and these characteristics he proved by reference to photographs of the several trees taken by himself at the time of observation. An examination was also instituted between these photographs and the drawings published by various travellers, from Ozonne and Humboldt, at the beginning of the century, down to Dr Herman Schacht in the present year; and the general conclusion was drawn, that no botanist, even although at the same time a great artist, should think of dispensing in the present day with the aid of photography in bringing home the facts and appearances of vegetable growth in distant lands.

2. *On the Structure of* Lemania fluviatilis. By Dr W. J. THOMSON.

The author remarks—" While recently employed in examining the minute anatomy of *Lemania fluviatilis*, I was struck by some points of structure which are not described in any work on the Algæ, and some of which are quite at variance with the descriptions and plates contained in Hassall's 'British Freshwater Algæ,' which plates are copied from Kutzing's 'Phycologia generalis.' The position of this genus, in a general classification of the Algæ, has hitherto been very unsatisfactory, but I believe that the points of structure about to be described will entitle it to a comparatively high and definite position among the Melanospermæ. The genus Lemania, as instituted by Bory, is characterized as having the frond attached, coriaceous, ramose, cellular ; outer cells small, polygonal, and firmly adherent ; interior larger, more lax, spherical, and empty. Now, this description, so far as it goes, is perfectly correct, but if a frond be allowed to macerate in water for some time, so as to destroy its colour, and render it more transparent, and then be slightly torn up with needles, and viewed with a magnifying power of 200 diameters, it will be seen to contain an axis consisting of a single articulated tube, which runs throughout the whole length of the frond, and bearing at each of the whorls of warts peculiar processes, to be afterwards described in connection with the organs of fructification. If the tips of the branches of the young and growing plants, as well as the warts, while the spores are coming to maturity, be examined by the same power, they will be seen to be covered with minute articulated, hyaline filaments, analogous to those which cover the young branches of *Desmarestia aculeata* and some other marine Algæ. But it is in connection with the organs of fructification that the most remarkable structure presents itself; the fructification is described thus:—'Sporules moniliform fasciculate, naked, arising from the inner vesicles, and occupying the interior of the frond.' Now, this is entirely erroneous. At each of the dilatations on the frond of the Lemania are from two to four wart-like tubercles; within each of these is found a bundle of branched moniliform filaments, which ultimately break up into spores. But these branches are not attached to the large internal cells, but to the central axis, by means of two to four elongated radiating cells, one to each tubercle, which are articulated to the axis by means of a round head (like that of the human humerus), the other end being expanded to a greater degree to receive the bundles of spore-bearing filaments. The upper extremity of the cells of the axis below that to which the radiating cells are attached, is also much dilated, so as to be at least three times the diameter of the lower end of the cell articulated to it superiorly. From the above characters, it will be at once seen that this genus has a very strong title to be classed in the natural order of Sporochnaceæ, in which it seems to occupy a position intermediate between Arthrocladia and Sporochnus, resembling the former in its articulated tubular axis, and in the arrangement of its spores in moniliform series, while it forms a transition by its consolidated knobs of filaments to the stalked receptacles of Sporochnus. In the articulated filaments which crown its growing apices and tubercles, it also corresponds with all the other genera of this order."

3. *Descriptions of some new species and varieties of Naviculæ, &c., observed in Californian Guano.* By R. K. Greville, LL.D., F.R.S.E., &c.

(This Paper has already appeared in the Journal.)

Thursday, April 14th.—Andrew Murray, Esq., President, in the Chair.

The following Communications were read:—

1. *On some of the Plants used for Food by the Fiji Islanders.* By Mr William Milne, late Botanical Collector in Captain Denham's Expedition to the South Seas.

The inhabitants of the Fiji Islands subsist mainly on the fruits of the earth. The principal articles of food are yams, taro, breadfruit, and bananas. Of yams there are upwards of fifty varieties in the islands. These grow to an enormous size, being sometimes 50 lb. or 80 lb. in weight; their general average, however, is from 2 lb. to 8 lb. They will keep for eight or ten months after being dug. The planting season extends from June to September, and the yams reach maturity in

March, and are dug in April. On the west coast of Naviti Levu two crops are secured, one in November, and another in March. The natives prepare the ground for cultivation by cutting down the natural vegetation, allowing it to dry, then burning it, and using the ashes as manure. They dig the ground with sticks, about six feet long, sharpened at the end. The smallest yams are used for planting, and sometimes the large ones are cut up like potatoes, and the pieces planted separately. When the yams begin to sprout, sticks and reeds are placed in the earth, and a rude frame is formed, on which the twining stems of the yams can support themselves. When the yams are ripe they are dug up and stored like potatoes. They are cooked either by boiling, baking, or roasting. The boiling is conducted in pottery ware of native manufacture, and is superintended by the women; baking is the work of the men. A large hole, from 9 to 18 feet in circumference, and 2 to 3 feet deep, forms the oven. This is filled with firewood, on which stones are laid. When these are thoroughly heated the oven is cleaned out, the stones being placed at the bottom and covered with leaves. The walls of the pit are lined with the same material. The yams, after being scraped, are laid on the stones and covered with several layers of leaves, and, finally, the oven is covered with a mound of earth. The yam, as well as other food, is served on wooden trays, which are covered with leaves. Another article of Fijian food is the Ndalo or *taro*, the root or rhizome of *Colocasia macrorhiza*. The principal crops are raised from November to April. The average weight of the root is 2 lb. There are two varieties, called land ndalo and water ndalo; the latter is that which is most generally grown. It requires irrigation, and valleys are selected for its cultivation through which a stream of water flows. The stalks and leaves of the plant are acrid, but in the young state they are used after preparation as articles of diet, either like spinach or in soup. The root is employed for making the mindrai or native bread. It contains much starch. The banana yields abundance of fruit from November till March. There are numerous varieties of it. The plant takes twelve months to arrive at maturity and bear fruit. The unripe bananas are boiled and baked, and the ripe ones are used as dessert. Sometimes they are made into puddings or bread. The *Musa Cavendishii*, or Chinese banana, has been introduced, and it bears abundance of fruit. The breadfruit tree (*Artocarpus incisa*) is another vegetable production of the Fiji Islands; it grows in abundance on the coast. The sweet potato or Kumera (*Convolvulus Batatas*) is also cultivated, and some other species of convolvulus. The Kawai, or sweet yam, is another edible root. The root of the Ki, or *Dracæna terminalis*, also yields food. It weighs from 10 lb. to 40 lb. Sugar-cane is eaten by the natives. The coco-nut is also employed as an article of diet, especially in times of scarcity. Among other fruits of these islands are Tarawan, a kind of plum; Kavika, or the Malay apple (*Eugenia Malaccensis*); and the Ivi, a kind of hog-plum (*Spondias dulcis*). The rhizome of *Tacca pinnatifida* yields a large quantity of starch, and is used as food after the acrid matter of the root has been removed by washing. The root or rhizome is grated, and after being steeped, is made into a kind of jelly, which is sweetened with the juice of the sugar-cane. From October to December there is sometimes a scarcity of food. The chief causes of want of food are improvidence on the part of the natives, war, and occasional failure of crops. Native bread is prepared from the taro, banana, kawai, breadfruit, and Tahitian chestnut. The roots and unripe fruit are scraped and bruised in pits thickly lined with banana leaves. These are covered over and left to ferment. The bread thus prepared may be kept for a long time. The material is taken out of the pit when wanted and made into cakes, which are wrapped in leaves, and then either boiled or baked. In general it may be said that the food of the Fijians during January consists of ndalo or taro and bananas; February, ndalo; March to September, yams and ndalo; October, breadfruit; November and December, ndalo and bananas. A great improvement has taken place in the habits of the natives since the introduction of Christianity.

2. *Extracts from correspondence between Dr Skene and Linnæus and John Ellis, about the year* 1765. Contributed by Mr Thomson of Banchory, and communicated by Professor Balfour.

3. *Notice of* Narthex Assafœtida (*the Assafœtida plant at present in flower in the Botanic Garden*). By Professor Balfour.

This season another of the Assafœtida plants in the Botanic Garden, raised from seeds sent home by Sir John M'Neil and Dr Falconer, has produced a flowering stem.

The specimen was planted out in front of the houses in the Garden about five years ago. It began to show symptoms of developing a flowering stem at the end of February and beginning of March; none of the large radical leaves were produced, but the flowering axis shot up at once from the underground stem. At the time when this took place none of the other specimens in the open ground of the Garden had shown any leaves. Warned by the untimely fate of the plant last year, which was suddenly destroyed by an intense frost on 13th April, when the thermometer fell to 22°, Mr M'Nab secured the present specimen from injury by getting a glazed wooden frame, about 8 feet high, erected around it, and connecting it with the adjoining stove, so that a moderate degree of heat might be supplied in the event of intense frost occurring during night. In this way the plant has been completely protected from the effects both of very high winds and of cold. It has progressed vigorously and rapidly. On the 13th April its height was 7 feet 8 inches. This height had been reached in about forty-five days. The last 30 inches of growth have been accomplished in eleven days—*i.e.*, from 2d to 13th April. The first anther expanded at 11 A.M. on 7th April, and in the course of that day the anthers appeared by hundreds. The plant has flowered well, and promises to bear fruit. At present there are 45 compound umbels on it, some of which are 5 or 6 inches across. The lower leaves on the axis have the characteristic pæony-like form, but they are by no means so large as those produced as radical leaves by the non-flowering plants. These leaves have compound laminæ 13 or 14 inches long, borne on evident rounded petioles, which at the base have short sheaths nearly surrounding the whole stem. The four lowest leaves do not bear umbels in the axil; all the rest do. In proceeding upwards the laminæ diminish in size, while the sheathing part of the petiole or the pericladium increases—the laminæ becoming 3½ or 4 inches long, the sheaths 7 to 9 inches by 8 inches in breadth. The laminæ in the upper leaves disappear, and the sheathing petiole alone is produced. Finally, near the top the sheaths are reduced to abortive membraneous scales about 1 inch in length, and at last they disappear entirely when we reach the umbels at the summit. From the axil of the sheathing petiole umbels are developed, those produced from the sheaths from the 7th to the 20th in ascending the stem being the largest. The peduncles of these compound umbels are 10–12 inches long, and the radii from 2–2½ inches, bearing perfect flowers. There are several sterile umbels showing stamens only, and no appearance of fruit. These are borne on long stalks, and have a more or less globular appearance. Small bractlets are seen on the peduncles of the general umbels, chiefly at the base of the abortive ones. These bractlets do not occur among the umbels. The odour of the flowers when expanded is of a sweetish honey-like nature, resembling that of *Galium verum*. From the whole plant, especially when bruised, there exhales a strong assafœtida odour. It yields a milky juice, which, when allowed to flow, concretes in clear tears on the stem, and has a very strong fœtid and enduring odour. The umbels come off from the axis alternately, but towards the middle and upper part of the axis they appear to have a somewhat verticillate aspect. Another plant which was transplanted last spring, and is now in the open ground behind the pits, has shown symptoms of flowering. It has begun to send up five flowering stems. The plant of last year which flowered, but was destroyed by frost before it fruited, died down, but the stem has sent out small lateral leaves within the last few days. It has not, therefore, died away after flowering, as is said to be the case in general. This occurrence may have depended on its being killed last year before it had gone through all its phases of flowering and fruiting.

Proceedings of the Royal Institution of Cornwall.

Shocks in Mountsbay on the days of the great Earthquakes of Lisbon in 1755, 1761, *and* 1858.* By R. Edmonds, Jun.†

Before I describe the recent shock at Tolvaddon, near Marazion, I would observe, that in Mr Mallet's "Catalogue of Recorded Earthquakes," published in the British Association Report for 1852 (p. 168), the earthquake of 1st November 1755 is stated to have been actually felt in the British Isles, only in Cork, Derbyshire, Berkshire, and Oxfordshire. But it was felt also in Cornwall and the Scilly Isles. Troutbeck, in his account of Scilly (1794), states, that on that day "several people ran out of their houses for fear they would fall upon them . . . and heard a noise like thunder, or the rattling of a wheel carriage upon a stony road at a distance. Several boats, which the tide had left dry, floated by the coming in of the sea so suddenly, that they rose several feet in a minute or two, and the sea went out again as quick as it came in, and left the boats dry again. It came in and went out again in the space of a few minutes" (p. 40). The fact, however, of its having been felt in various parts of Cornwall, rests merely on common tradition. The late Rev. Canon Rogers, after hearing a paper on earthquakes read before the Royal Geological Society of Cornwall in 1855,* stated to the Society that one of his ancestors at Helston heard the sound accompanying it, which resembled that of a carriage passing.

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* Read at Truro before the Royal Institution of Cornwall, May 13, 1859.

† [Mr Edmonds has contributed several papers on similar subjects which have appeared in the Journal. In the number for January 1858, p. 146, his name is by mistake printed *Edwards*.]

Tolvaddon Mine (where the recent shock was felt) is situated on high ground, a mile north-east of St Michael's Mount—a picturesque pyramid of granite protruding through the slate formation, which extends in every direction continuously for miles around it. The shock was observed only on the *floors*, a paved horizontal plot of ground on the surface of the mine. The weather at the time was very gloomy, the wind about south-east and squally. Captain Francis Gundry at 12h. 20m., being then on the *floors* a few yards from the Count House, felt the ground tremble beneath him for two or three seconds, and after an interval of three or four seconds the tremor was repeated, both tremors being accompanied with sounds proceeding from the south-west, not unlike the noise of explosions of distant cannons at sea. The motion and noise were experienced also by another person then standing on the same floors, about forty yards west of Captain Gundry. The sound was heard by a third individual, who, although very near the floors, felt no movement of the earth. These three persons stated the above facts to me when I visited the mine nearly a month afterwards, but although they did not remember whether the day was the 10th or the 11th of November last, they were very certain that the time of the day when it occurred was 12h. 20m., as that was the Captain's usual dinner-time, and he was then going into the Count House to dine. The day, however, may, I think, be fairly assumed to have been the 11th, not only because there was a great earthquake on that day throughout Spain and Portugal, and beneath the Atlantic, but also because there would in that case be an interval of the same number of hours between each of the great shocks at Lisbon in 1755, 1761, and 1858, and a shock or agitation of the sea at or near St Michael's Mount, as will be shown in the next paragraph.

Four hours and five minutes after the *first great earthquake* at Lisbon (1st November 1755), an extraordinary agitation of the sea commenced at St Michael's Mount. Four hours and forty minutes after the *second great earthquake* at Lisbon (31st March 1761), another such agitation commenced at the Mount. Four hours and fifty minutes after the *third great earthquake* at Lisbon (11th November 1858),† a shock, as already mentioned, was felt about a mile from the Mount on Tolvaddon Mine. The time on each of these occasions was thus four hours and a fraction after the great shock at Lisbon. The place which suffered most from the earthquakes of 1761 and 1858 is St Ubes, twenty-two miles south-east of Lisbon, and severe shocks were felt at sea many leagues off Cape

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* Edinburgh Philosophical Journal for April 1856.

† This, according to the *Times* Newspaper of 23d November, was felt in Lisbon at 7h. 15m. A.M., which, and 15m. for the difference of longitude, equal 7h. 30m. by Mountsbay time, 4h. 50m. after which (or twenty minutes after noon) the shock was felt at Tolvaddon.

St Vincent in 1858[*] and 1755, indicating that the centre of disturbance on each occasion was beneath the ocean, some distance westward of the coast of Portugal, from about which direction the sound heard at Tolvaddon seemed to proceed.

This last earthquake at Lisbon was preceded by two days of almost incessant heavy rain, and at the time it took place the wind was rather fresh from E.S.E., with a heavy and rainy atmosphere. During the day the wind changed to the south-west whence a sharp gale was experienced, which, during the evening, drove the French steamer "Ville de Malaga" from her moorings opposite the Lisbon Custom-house.

This recent earthquake of Lisbon, and the shock at Tolvaddon, were two days before the moon's first quarter. And it is most remarkable that all the twelve recorded earthquakes and extraordinary agitations of the sea in the Landsend district and the Scilly Isles during the present century, have been (with the exception of the very limited ones—extending only a furlong or two—of 30th May and 6th June 1855) nearer to the moon's first quarter than to any other.[†]

Penzance, 13th May 1859.

SCIENTIFIC INTELLIGENCE.

BOTANY.

Cola Nuts of Africa.—They are the produce of *Sterculia acuminata.* Immense quantities of these nuts are carried during the dry season from the coast to the interior of Africa. During half the year caravans pass Rabba on the Kevorra; and about 1000 donkeys monthly are laden with cola nuts. These are carried pannier-fashion, two baskets with each animal, each basket containing 50 lb. weight. Cola nuts are not very often carried in the pod, being in this form too cumbersome; but as it is necessary that they should be kept moist, the baskets are well protected with leaves of a species of Phrynium. The variety of cola called Gonga in the Nupe country is the most prized, being worth about 100 cowries each; the value of cowries at Rabba being 2500 for the dollar of 4s. 4d. Bitter cola is used for medicinal purposes. It is intensely bitter, not astringent like common cola. Bitter cola fruit is about the size of a peach, rose-coloured, and very pretty.

On the Temperature of Plants. By M. Becquerel.

M. Becquerel has recently made observations on the temperature of plants, by which he shows that the calorific condition of plants depends on the air, and that, like the atmosphere, it is liable to diurnal, monthly, and annual variations.

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[*] This shock (according to the newspapers) was felt fifty miles off the Cape on board the ship "Istok," bound from Alexandria to Liverpool.

[†] The dates of the shocks are 30th December 1832; 21st January1839; 17th February 1842; 30th May 1855; 11th November 1858.

The dates of the oscillations are 31st May 1811; 5th July and 30th October 1843; 5th July and 1st August 1846; 23d May 1847; 6th June 1855. (*Transactions of Geological Society of Cornwall* for 1843 and 1844, pp. 112, 208, and 1855, p. 279.)

The first experiments on the heat of plants which attracted the attention of botanists were those of Hunter, published in the "Philosophical Transactions" for 1775 and 1778. He simply introduced a thermometer into a hole made in the trunk of a tree, protecting the stem of the thermometer from external influences by means of a box filled with wool, through which the thermometer passed. This arrangement did not prevent the entrance of air and water, and hence resulted errors in the observations. The experiments were also of a very limited extent, and no indication is given of the hours of the day when they were made. Hunter concluded that in winter trees had a temperature above that of the air.

In 1783, Schoeffs made experiments on the subject in New York under still less favourable conditions, since he operated on trees of different diameters and of different kinds, with thermometers which he introduced during some minutes only into cavities prepared for the purpose. The observations made by him show only that the temperature of trees was sometimes higher, sometimes lower, than that of the air.

At Geneva, from 1796 to 1800, and for some years afterwards, Pictet and Maurice made a continuous series of observations on temperature, at sunrise, at 2 P.M., and at sunset, on a horse-chestnut, which was about 1 foot 9 inches in diameter.

The cavity in which the thermometer was placed was filled with melted tallow, in order to prevent the access of air and water. They made during the period mentioned 11,000 observations, the results of which were registered by them without any discussion of the subject. M. Becquerel has taken up these observations, and has drawn conclusions from them. He has also constructed curves representing the results.

During the years from 1795–1800, the mean annual temperature of the air in the north was the same as that of the tree, the differences being not more than one or two tenths of a degree; and these may be attributed to the displacement of the zero or basis of observation.

Founding on a certain series of observations, it had been supposed that the tree had a temperature more elevated than that of the air during winter, and lower during summer, and it had been concluded from this that the effects were due to this,viz., that the fluids drawn up by the roots were themselves warmer than the air in winter, and colder in summer. This theory, however, is shown to be inadmissible, so far as the results of the Geneva observations are concerned. Thus, in the years 1796, 1798, and 1799, during the months of May to August, the temperature of the tree was decidedly above that of the air; but the contrary was the case during the years 1797 and 1800, with about two exceptions. On the other hand, during the winters 1796–1797, 1797–98, 1798–99, the tree was colder than the air.

Observations have also been made at Geneva relative to the temperature of the earth, at about 4 feet of depth—a depth to which the principal roots penetrate. M. Becquerel concludes from this that the temperature of the soil does not produce any influence on the temperature of the tree. During the years 1796, 1797, 1798, and 1799, the temperature of the soil in winter was higher than that of the air and of the tree; it was lower in spring; and again it was higher in summer and autumn. The water absorbed by the roots does not therefore appear to affect the temperature of the tree. M. Becquerel therefore concludes that the cause of plant temperature must be looked for in the air.

He finds that the curves of mean temperatures of the air present great inflexions, while those of the tree are much more uniform in their course. The curves and variations show that the hours of maximum and minimum are not the same in the air as in the tree. The maximum of the air occurred, according to the season, between 2 and 3 P.M., whilst in the tree it showed itself a considerable time after sunset.

In the month of December last, M. Becquerel made observations on a horse-chestnut in the Jardin des Plantes, having a diameter of about 1 foot 8 inches. He made holes 11, 6½, and 5½ inches in depth in the trunk of the tree, at the height of 1 metre above the ground. Into these holes were introduced mercurial and electric thermometers. The hollows were filled with melted tallow. The parts of the instruments outside the tree were protected from the variation of temperature in the air, in order to be certain that they did not exercise any influence on the temperature indicated by the thermometers. It was ascertained by experiment that the changes of temperature in the portions of the thermometers in contact with the air did not modify in any degree the temperature of the tree when that of the air changed; since by maintaining at zero, by means of melting ice for forty-eight hours, the stem of one of the mercurial thermometers, it was found that the same thermometer gave indications similar to those of others placed at the same depth in the tree. Comparative observations made during the months of December, January, February, and March last have given results from which the following conclusions are drawn.

1. The mean temperature of the air and the tree have been sensibly the same, a result which has been deduced equally from the observations made at Geneva from 1796 to 1810, and at Chatillon-sur-Loing last summer. The result is the same whatever the diameter of the tree is; only the smaller the diameter, the more quickly the equilibrium of temperature is established. In the leaves it takes place at the end of a short period, in the branches and small twigs a little more slowly, then in the thick branches and the trunk, and finally in the root. When there are great variations of temperature in the air, complex effects are produced on the tree, which disappear, however, in taking the means.

2. The production of heat resulting from chemical reactions taking place in the vegetable tissues, seems to account only for an inappreciable portion of heat in plants. Such is also the case with regard to the porper heat of fluids absorbed by the roots, and which afterwards form the sap.

3. During the months of December, January, and February, the mean variations of temperature in the air from 9 A.M. to 9 P.M. was 0°·84, in the tree 0°·19 at the depth of 6½ inches, and 0·10 at the depth of 11 inches. The variations at these depths have thus been six times and eight times less than in the air.

4. The maximum of temperature in the air took place in winter about 2 P.M., and in the tree about 9 P.M., and only about midnight in summer.

5. The transmission of heat takes place gradually from the periphery

to the centre in a definite time, which can be determined by electric thermometers, placed at different depths in the tree.

6. The atmosphere is the natural source whence plants derive the heat which constitutes their caloric condition, and which they require in order to perform their functions. They are in the same condition as fishes which possess sensibly the same temperature as the medium in which they live. As these animals, however, have the power of locomotion, they can protect themselves from variations, by approaching the surface and going deeper into the water at pleasure. Plants, on the contrary, are forced to submit to the temperature of the medium in which they grow, and cannot withdraw themselves from it.

Remarks on some Plants and their Natural History Contributions from Old Calabar. By ANDREW MURRAY, Esq.

Mr Murray, in giving a *resumé* of the different contributions to various branches of natural History, which had been made to himself and others by the United Presbyterian Missionaries at Old Calabar, remarked, that the Rev. Mr Hope N. Waddell, the Rev. Mr Goldie, the Rev. Mr Thomson, Mr Hewan, Mr Baillie, and the late Mr Wylie, had all contributed largely to science. It was through them that the interesting electric fish, the *Malapterurus Beninensis* had been first made known, and subsequently had, on various occasions, been introduced alive into this country. When the first living specimens arrived, Professor Goodsir (to whom they had been sent), with that total abnegation of self and pure devotion to science which so strongly characterizes him, took them to Berlin, to place them at the disposal of Professor Du Bois Raymond, who was engaged upon the third volume of his great work on Electricity, being the volume which related to animal electricity. The value which was attached to this opportunity of studying the phenomena of electricity shown by these animals may be estimated by the fact that after these specimens died (which, whether from over-experimenting, or unsuitable management, soon happened), the Berlin gentleman made interest to get more specimens, but in a way which may be thought illustrative of the difference between the two nations in relation to private enterprise as compared with that of the Crown. They applied to Prince Albert, through the royal family of Prussia. It is unnecessary to say that Prince Albert could donothing; but what he was unable to do, we were soon enabled to do here by receiving a second consignment, addressed in this instance to Professor Balfour, who, not less liberal, nor less devoted to science than Professor Goodsir, willingly sacrificed them to Professor Du Bois Raymond. Others have since been received, and Mr Hewan has brought two with him on his return from Old Calabar about a month ago. These were placed in one of the tanks in the Botanic Garden of Edinburgh. One of them died, but the other is still alive and healthy. Mr Murray enumerated various other novelties in science which had been sent home by the missionaries, and had been described by himself and other naturalists. Until now their contributions had been chiefly confined to zoology, but last month he had received a box from the Rev. W. C. Thomson, containing a number of seeds and botanical specimens collected at Ikoneto, some of which possessed much interest. The seeds he had handed over to Mr Macnab, who had sown them, and symptoms of germination had already begun to appear. The botanical specimens he had presented to the Museum of the Botanic Garden. The most interesting of these was a number of specimens of the poison ordeal bean of Old Calabar, which had been first brought home a number of years ago by the Rev. Mr Waddell. It was then analysed and examined by Professor Christison, who reversed the adage of *fiat experimentum in corpore vili*, by experimenting upon himself, and nearly making a vacancy in the Chair of Materia Medica in the University. His experiments and analysis were published in the "Proceedings of the Royal Society." The bean was used as an ordeal by the natives, and it was undoubted that some escaped, while to others it was fatal. It had been suggested that this might arise from the fetish men or priests who administered the poison, causing those whom they had destined to death to take a smaller dose than those who were to escape, because when taken in a large dose it occasioned vomiting, which might relieve the stomach of its perilous inmate. The seeds brought by Mr Waddell had been grown by Mr Macnab, but had died before flowering. It has not yet been described, and Mr Murray now gave a description of it, so far as the materials sent to this country allowed. It appeared to him to be a Mucuna, and he designated it *Mucuna venenosa.* The Mucuna was well known as the genus which furnished the drug called Cow-itch, which was derived from the hairs with which the pod is covered, and was used as an anthelmintic. The larger seeds, often thrown on the west coast of Ireland, are the seed of the West Indian *Mucuna pruriens.* No species from Africa has yet been described. There is, however, a species, besides the poison bean, which has just been sent by Mr Thomson, very distinct from the West Indian species, but very closely allied to a species from Tranquebar. Had the latter been a native of the West Indies, instead of the East, he would have had some hesitation in describing this as new, as it might have been floated across to Africa from the West Indies, and so become naturalized there; but it was difficult to believe that this could be the case with an East Indian species. He therefore described this species under the name of *Mucuna Balfouriana*, after Professor Balfour, who had had specimens of this unnamed standing in the Museum for many years. There were several other most interesting specimens which botanists seemed to be unacquainted with, and had difficulty in localising. But as the specimens only consisted of the fruits and seeds, the materials were too scanty to allow of their being described and named.

Mr Hewan, in reference to Mr Murray's remarks regarding the poison ordeal bean, stated that the natives generally did not put much faith in its being a true ordeal, rather looking upon a summons to undergo the test as sentence of death, and, if in their power, making their escape and going into exile. He did not think the suggestion that the larger dose producing vomiting was the true explanation why some escaped while others did not. In one case which had come under his own notice a short time ago, a woman, who was accused of injuring her child by witchcraft, came in a from distance strong in innocence, and demanded to have the ordeal administered. She ate twenty-four beans and did not die. Next day another woman, encouraged by her escape, underwent the ordeal, and she ate twenty-two beans and died. The difference in quantity here was too slight to affect the result. He rather thought it was from the mode of preparing the beans that the different results followed. The beans were steeped before being eaten, and as the fetish man had the preparation of them, he could, if he chose, boil all the poison, or much of it, out of them before administering them.—*Proceedings of Botanical Society of Edinburgh.*

CHEMISTRY.

Japanese Wax.—Professor W. B. Rogers states that the Japanese wax, though as white as bleached bees-wax, at ordinary temperatures, is more brittle, less ductile, and breaks with a smoother and more conchoidal fracture; its specific gravity is slightly less, and its melting-point about 127°. Like bees-wax, it is separable into three fatty bodies, whose proportions in round numbers are, in 100 parts—soluble in cold alcohol (60° F.), 12 parts; in hot, 55 parts; and insoluble in alcohol, 33 parts. Bees-wax similarly treated yields respectively 4 or 5, 22, and 73 or 74 parts of the ingredients, which are called cerolein, cerotic acid, and myricine; the first two fatty acids, and the last a neutral fat compounded of palmitic acid and a fatty base. The three corresponding substances obtained from the vegetable wax differ from the above in their physical properties, and may on examination be found to consist wholly or in part of distinct and perhaps new fatty bodies. In regard to its economical applications, it may be added that the great readiness with which it is saponified, and the clear and strong light which it yields when burned in the form of candles, give promise that it may ere long become an article of considerable commercial importance.—*Proceedings of the Boston Society of Natural History.*

GEOLOGY.

Pteraspis in Lower Ludlow Rock.—The President of the Malvern Natural History Field Club, the Rev. W. S. Symonds, announced at the Apperley meeting of the Cotteswold, Malvern, and Warwickshire Field Club, the discovery of that oldest known fossil fish, the Pteraspis, in the Lower Ludlow rock of Leintwardine, near Ludlow.

Beyrichia.—Beyrichias have been found by Mr Robert Jones in specimens sent to him by Professor Dawson of Canada, from the lower carboniferous beds of Nova Scotia. They have never hitherto been met with in any later formation than the Upper Silurian. Mr Jones has also seen Beyrichias in the lower carboniferous strata of the border counties of North Britain, in specimens with which he has been favoured by Mr Tate of Alnwick. A new genus, allied to Beyrichia, and to be named Kirkbya, is represented by one species in the lower carboniferous rocks of Glasgow, and another in the magnesian limestone of Sunderland.

MINERALOGY.

The Mineral Kingdom, with Coloured Illustrations of the most important Minerals. By Dr J. G. KURR.

Among the many attempts recently made to facilitate the acquisition of science, and render it attractive, we have not met with one more commendably conceived and executed than this neat folio volume on the

Mineral Kingdom, by Professor Kurr of Stuttgart, and now published in English, in a very handsome style, by Edmonston and Douglas of Edinburgh. The leading object of the work is to assist those who aspire to a knowledge of mineralogy to surmount the difficulties which beset them at the commencement of the study, by furnishing "coloured illustrations of the more important minerals," in elucidation of a clearly-written descriptive text, where the intrinsic interest of the details is never marred by a too technical arrangement and phraseology. This mode of inviting to the study of mineralogy is undoubtedly a sound one, in obvious accordance with approved principles of imparting instruction. It aspires to as high a function as a book can perform in teaching natural science, replacing the actual objects by faithful and pleasing representations of choice, well-selected specimens, and supplying in accurate and yet unpedantic language the absence of a living, well-informed instructor. Undoubtedly the easiest path to exact knowledge is that of the fortunate student who, with the things of nature before him, is guided to their features and properties by an accomplished oral teacher. But few are privileged to command the instruction of a competent tutor with a good cabinet; and every one who has tried it, or witnessed its effort, knows how obstructed and painful are the steps of the beginner in natural history, who has no better aid than that of a mere collection of specimens in an unillustrated text-book. The work before us is, in its illustrations, in some respects better for the beginner than an ordinary cabinet, inasmuch as it enables him to become familiar with the objects under those distructive and instinctive forms which are rare, costly, and not easily met with. On the other hand, it is preferable to the common treatises in assisting the student to a clearer understanding of the more than ordinarily lucid scientific text, by its copious series of carefully-drawn and faithfully-coloured likenesses of the minerals described, most of which are indeed beautiful portraits of actual specimens.

It begins with a concise but sufficiently full "introduction," treating of the general properties of minerals, to which is appended a most useful tabular view of their chemical constituents, showing the properties of these and their modes of occurrence. "Special Mineralogy" occupies the remainder of the book, in fourteen well-arranged chapters, elucidated by constant references to the brilliantly-painted representations of specimens, and the linear diagrams of crystalline forms. The first chapter, devoted to precious stones and gems, is full of interesting details, conveyed with the requisite scientific precision, but unobscured with needless technicalities of language. Its attractive descriptions are enforced upon the memory by about 120 superbly-tinted figures, embraced in five folio plates. The final chapter, treating of the heavy metals and metallic minerals or ores, is not less splendidly and copiously illustrated, having no fewer than *ten* plates, covered by 203 very richly-coloured portraits of specimens appropriated to it.

Notwithstanding the unavoidable costliness of such delicately-executed hand-coloured plates, the publishers have not stinted the work in this its most valuable and attractive characteristic, but have supplied no less than 24 of these tables, as the Germans call them, embracing 468 figures.

We hail this book as a successful application of coloured engraving to a department of natural history which hitherto has received but little aid from this expressive sister art to printing. To the extent to which it professes to unfold the structure of the mineral kingdom, the work has the qualities of accuracy and truthfulness to the science of the day, which entitle it to the favour of the real student; while its brilliant and tasteful delineations of a most pleasing and curious assemblage of objects, give it an equal claim to the consideration of every lover of the beautiful in nature.

MISCELLANEOUS.

The Magnetic Telegraph Foreshadowed.—In "Bailey's Dictionary," edition of 1730,—127 years ago,—under the word "Loadstone," is the following foreshadowing of the electric telegraph:—"Some authors write that, by the help of the magnet or loadstone, persons may communicate their minds to a friend at a great distance; as suppose one to be at London and the other at Paris, if each of them have a circular alphabet, like the dial-plate of a clock, and a needle touched with one magnet; then at the same time that the needle at London was moved, that at Paris would move in like manner, provided each party had secret notes for dividing words, and the observation was made at a set hour either of the day or of the night; and when one party would inform the other of any matter, he is to move the needle to those letters that will form the words, that will declare what he would have the other one know, and the other needle will move in the same manner. This may be done reciprocally."

To the Editors of the Edinburgh New Philosophical Journal.

21 RUTLAND STREET, EDINBURGH,
13th May 1859.

GENTLEMEN,—Having published in your valued journal the Table of the Rain-fall in Scotland during the years 1856 and 1857, I now beg to hand you that for 1858, containing the returns from sixty stations, being all those from which the returns were complete. For the sake of comparison, the mean results for the years 1856 and 1857 have been appended; and from these it will be seen how very closely the annual rain-fall over the country corresponds one year with another, and justifies the conclusion I arrived at last year, that the mode in which the rain falls, and the months during which it is precipitated, have more to do with the character of the year, as a wet or a dry one, than the mere quantity of water which is deposited.

In the rainy year 1856, least rain fell during March; while in the dry year 1857, least fell during May. In 1858, again, February was the driest month, and March and April the next. In 1856 the greatest quantity of rain fell in December, and the next greatest in September; in 1857, the greatest fall occurred in September, and the next greatest in December. During 1858, however, by far the greatest deposit of rain occurred in October, and the next greatest in July; the fall in December, however, very nearly approaching that of July.

On looking over the returns from the different stations, it will be seen, as was pointed out last year, that the nearer the station is to a high hill or elevated range of mountains crossing the line of the prevalent wind, and the more elevated that hill or range of mountains, the greater has been the deposit of rain.—I have the honour to be, &c.,

JAMES STARK.

Fall of Rain at 60 Stations in Scotland during each Month of the Year 1858.

STATION.	Jan.	Feb.	Mar.	April.	May.	June.	July.	Aug.	Sep.	Oct.	Nov.	Dec.	TOTAL
Sandwick	3·78	0·74	2·71	0·86	2·43	2·24	4·05	2·64	2·80	5·89	2·83	3·40	34·37
Tongue	5·90	1·60	3·15	3·00	2·40	2·50	6·50	3·60	2·00	6·50	3·30	2·00	42·45
Stornoway	6·61	2·22	2·78	0·62	2·72	3·30	4·40	1·79	4·98	3·70	2·07	4·42	39·61
Culloden	2·70	0·38	1·73	0·81	2·55	2·35	4·17	3·20	0·89	2·79	0·95	2·27	24·79
Elgin	1·99	0·61	3·16	0·89	3·11	1·35	4·71	2·01	1·48	3·18	1·12	1·60	25·21
Castle New	1·42	1·30	3·20	0·90	2·35	1·07	4·63	2·16	1·08	3·38	0·99	2·61	25·09
Braemar	2·35	1·02	2·16	1·30	2·75	1·47	4·87	2·00	1·55	2·74	2·29	3·36	27·86
Banchory	1·10	0·60	2·20	0·60	1·50	0·90	3·60	3·00	1·30	4·80	4·00	5·30	28·90
Aberdeen	1·16	0·62	2·53	0·90	1·84	1·36	3·60	2·85	1·25	4·85	3·85	4·15	28·96
Fettercairn	0·90	0·90	1·60	0·80	1·30	1·30	3·80	1·80	2·30	3·00	3·10	4·20	25·00
Montrose	0·60	0·70	1·10	0·85	1·80	2·32	3·10	2·27	1·85	2·62	2·20	3·00	22·41
Arbroath	0·92	1·04	1·28	1·28	2·09	1·51	3·07	2·27	2·02	2·66	1·87	4·31	24·32
Barry	1·25	1·09	1·23	1·25	1·97	1·32	3·75	2·44	2·76	3·28	3·01	3·47	26·82
Kettins	1·51	1·20	1·20	1·55	2·42	1·77	6·74	3·10	2·85	3·12	2·18	3·45	31·09
Perth	2·22	1·02	1·25	1·97	2·32	1·97	6·98	2·75	2·96	3·78	1·87	3·45	32·54
Trinity-Gask	2·50	0·90	1·40	2·40	3·60	1·80	5·00	1·90	3·10	4·40	1·40	4·70	33·10
Taymouth	4·40	1·30	1·40	2·70	2·20	2·20	5·20	2·60	3·10	3·50	2·20	5·80	36·60
Tyndrum	13·48	3·00	3·45	4·17	7·22	5·84	5·54	5·67	5·83	8·50	2·48	8·98	74·16
Pittenweem	1·35	0·90	0·97	1·44	2·04	1·54	3·36	2·38	2·16	3·25	2·73	2·95	25·07
Nookton	1·49	0·99	1·16	1·43	2·25	1·19	5·42	2·22	2·73	3·05	1·94	3·85	27·72
Balfour	1·78	1·07	1·17	0·95	2·11	1·67	4·24	2·63	2·66	3·40	2·27	3·87	27·82
Stirling	3·79	0·70	0·89	2·24	4·34	2·39	4·86	3·17	2·86	6·22	2·15	4·35	37·96
Millfield	3·67	1·92	1·41	2·15	3·30	2·49	5·35	2·81	3·81	4·92	1·50	3·27	36·60
Callton Mor	4·49	0·79	3·10	3·83	4·45	5·47	3·66	6·00	3·31	5·15	1·36	9·76	51·37
Easdale	6·00	1·20	4·90	3·90	4·80	5·90	4·90	4·70	3·60	7·50	0·80	7·50	55·20
Moile	4·53	2·75	3·15	8·90	4·57	3·30	2·70	4·70	3·83	5·60	3·43	12·80	60·26
Gadgirth	3·10	0·90	2·98	3·45	3·80	3·80	4·30	1·80	4·00	5·45	1·90	5·20	40·68
Greenock	7·60	1·40	3·00	4·85	5·30	3·55	4·70	3·20	4·10	8·25	2·10	9·85	57·90
Glasgow	3·67	1·29	3·05	2·66	3·76	5·58	6·85	3·04	4·16	5·91	2·10	4·05	46·12
Baillieston	3·02	0·95	1·89	2·03	3·21	3·28	4·47	2·28	1·60	4·80	2·05	2·59	32·17
Newliston	1·80	1·90	0·80	0·70	1·90	1·80	4·30	0·80	1·70	2·80	1·40	3·80	23·70
Harlaw	2·60	1·50	1·30	1·60	2·80	1·50	5·00	2·40	3·00	6·60	2·02	2·00	32·42
Swanston	2·60	1·43	1·42	1·04	2·80	1·70	4·83	2·25	2·80	6·60	2·03	2·00	31·50
Glencorse	2·55	1·10	1·60	1·25	2·95	1·70	4·00	1·75	2·25	4·70	1·35	2·45	27·65
Edinburgh	0·99	1·08	0·79	0·85	0·98	2·48	4·16	2·42	1·93	3·92	1·64	1·43	22·67
Smeaton	1·17	0·74	1·03	1·12	1·70	0·89	3·53	2·25	1·21	3·21	2·99	1·35	21·19
East Linton	1·40	0·90	1·88	1·12	2·20	1·01	3·74	2·20	1·34	3·58	2·29	1·66	23·32
Thurston	0·40	0·40	0·90	0·90	3·20	1·20	2·20	1·50	2·50	4·50	4·50	1·80	24·00
Yester	0·80	0·80	2·10	1·90	3·55	1·45	6·05	0·60	1·50	5·00	1·93	2·45	28·13
Thirlestane	1·40	0·70	1·30	1·76	2·46	1·60	3·96	1·95	2·85	2·55	5·71	2·25	28·49
Mungo's Walls	0·72	0·52	0·88	0·94	1·78	1·28	3·54	1·15	2·13	3·71	3·26	1·69	21·60
Milne-Graden	0·60	0·52	2·20	1·20	3·30	2·90	5·10	2·00	3·00	3·30	3·70	1·90	29·72
Stobo	1·40	0·70	0·30	0·50	2·50	1·10	1·90	1·30	1·30	3·20	1·50	1·70	17·40
Bowhill	2·15	0·86	1·83	1·04	2·86	0·66	2·86	1·90	4·78	4·90	2·14	1·35	28·33
Makerstoun	1·60	0·39	0·93	0·91	2·35	1·38	4·49	1·35	2·66	3·52	1·05	2·33	22·96
Drumlanrig	4·50	2·60	1·40	3·60	5·30	3·10	4·50	4·50	4·40	6·50	2·00	5·60	48·00
Kirkpatrick-Juxta	5·30	1·38	2·55	3·15	6·45	2·15	4·70	4·20	6·60	7·80	2·80	6·10	53·18
Thornhill	3·90	1·00	1·20	3·80	4·60	3·90	4·00	4·00	4·00	6·30	1·50	5·70	43·90
Penpont	3·70	0·70	1·20	1·90	4·50	3·60	4·00	4·20	4·60	6·40	1·80	4·90	41·50
Keir	3·60	1·40	2·70	3·65	5·20	2·80	4·00	4·60	5·00	7·50	2·70	5·00	48·15
Auchenbrack	5·40	0·80	2·20	2·20	5·50	3·50	4·20	3·00	6·50	7·80	2·00	4·50	47·60
Hastings Hall	6·30	2·00	3·10	3·30	6·10	4·00	4·00	6·85	6·70	8·90	1·50	4·50	57·25
Kirkconnell	4·40	0·90	2·90	2·20	3·80	4·20	4·10	3·40	4·10	6·80	2·00	5·80	44·60
Wanlockhead	5·95	1·30	3·25	2·05	6·90	3·65	5·75	3·45	5·85	7·85	3·55	8·15	57·70
Canonbie	1·80	0·20	2·10	1·70	2·40	1·60	3·70	4·10	3·70	5·00	2·40	1·90	30·60
Langholm	5·50	0·50	2·50	1·50	3·10	3·50	3·50	6·00	3·50	7·70	4·00	3·80	44·60
Ewes	3·30	1·20	1·90	2·60	3·70	2·20	2·60	6·70	4·80	6·40	3·70	6·80	45·90
Westerkirk	4·50	0·80	3·30	3·00	5·20	1·20	4·00	6·40	7·40	7·80	2·30	6·70	52·60
Carlesgill	4·70	1·00	3·50	2·40	5·75	2·20	4·20	8·25	7·20	8·00	2·40	6·30	55·90
Eskdalemuir	3·10	4·00	2·80	3·60	6·00	1·02	4·10	7·00	8·80	7·60	1·80	5·40	55·22
Mean, 1858	3·12	1·14	2·01	2·03	3·37	2·36	4·35	3·15	3·35	5·12	2·33	4·20	36·53
Mean, 1857	3·29	2·14	3·41	2·40	1·85	3·08	2·48	2·28	4·39	2·57	3·05	3·96	34·90
Mean, 1856	2·86	3·55	0·37	2·69	2·96	4·36	2·64	4·00	4·79	1·91	1·98	4·85	36·96

On Professor Hughes's System of Type-Printing Telegraphs and Methods of Insulation, with general reference to Submarine Cables. By Mr Hyde.—The author, after calling attention to the vast importance of perfect insulation, said, that although gutta percha had been found to be the best insulating medium for long submarine lines, yet as this substance was more or less porous, minute flaws might exist, which did not show themselves until some time after the immersion of a cable. To meet these defects, to fill up any minute pores in the gutta percha, and also to cure any accidental fracture or puncture of it, Professor Hughes introduced a viscid semi-fluid substance, of a non-conducting character, between the conducting wire and the gutta percha; or the wire might first be coated with gutta percha, and the viscid fluid introduced between the layers of gutta percha. As soon as a puncture was made in the gutta percha coating, this fluid oozed out, which was of such a nature that it hardened when it came in contact with the surrounding water. This hardening property allowed no more of the fluid to ooze out than was necessary to fill the fracture, and at the same time to glue and unite the separated parts of the gutta percha. The author then proceeded to speak of the various telegraphic instruments used, referring especially to the wording instrument of Morie and House, and explained the type-printing instrument of Professor Hughes, which is worked by means of 28 keys, arranged like those of a pianoforte. These keys correspond to 28 holes, arranged in a circle on the table of the instrument. Each key is connected by a lever with a cast-steel knob, which, when the key is pressed down by the finger, rises up through one of the holes. An arm, driven by clockwork, connected with a vertical shaft, sweeps over the 28 holes; when a key marked with a particular letter is touched, the knob corresponding with this letter rises, the revolving arm passes over it, and for the instant closes the circuit, and allows an electrical impulse to be transmitted. This impulse causes the particular letter to be recorded on a slip of paper, in printer's ink, by means of a type-wheel connected with the machine, which lifts the press and the paper upon which the message is to be printed against it. The time of the lasting of the locking of the shafts depends upon the arrival of the electrical wire; and thus with two instruments in perfect harmony, the operator has the printing apparatus of the distant instrument as completely under his direction as the one before him. The instruments at each end of the line are adjusted by means of spring pendulums, to work synchronously; but in order to correct any minute variation in time between the instruments in circuit, there is a corrector, or wheel, attached to the shaft, with hook-shaped teeth, which sink into corresponding cavities in the type-wheel. The latter being loose upon the slip, or only held by friction, is removed backward or forward by the corrector, or exactly the same route as the type-wheel, on the instrument from which the message is being sent. This correction takes place in the act of printing every letter. Mr Hyde stated that European news, consisting of about 3000 words, by the arrival of each transatlantic steamer, is transmitted by this instrument from Boston to New York, a circuit of about 300 miles, at the rate of 2000 to 2500 unabbreviated words an hour. There are 25 stations on the circuit which receive copies of the news, all of which are printed in plain Roman type by the Boston operator, all the instruments receiving the message at the same time, the receiving clerks of each station having simply to hand the slip as it arrives to the party entitled to receive it. —*Practical Mechanics' Journal, London.*

Large Nugget of Gold.—At a recent soiree of the Royal Institution, Professor Tennant exhibited an unusually large and beautiful lump of native gold, brought last year from the Gingomer Diggings, 120 miles from Melbourne. When melted (August 4, 1858) it yielded L.6905, 12s. 9d. Amounts of gold received from Australia:—In 1855, 125 tons; 1856, 147 tons; 1857, 115 tons; 1858, 106 tons.

On Changes produced by the deepening and extension of Mines on the temperatures at their previous bottoms. By WILLIAM JORY HENWOOD, F.R.S., F.G.S.*—In 1805–7, M. de Trebra determined, by observations at intervals of eight hours daily, that at unfrequented parts in the second and sixth levels of the *Beschert Glück* mine in Saxony, the air had not varied in temperature for two years; and that it was warmest at the deepest spot; having remained in the former at 52°·25, and in the latter—270 feet deeper—at 59°.†

In 1820–2, Mr Fox ascertained that for nearly twenty months a hole in the rock at the deepest level in *Dolcoath*—about 231 fathoms from the surface—maintained "the constant temperature of 75°·5."‡

These facts indicate that, whilst the conditions of works in mines are unaltered, their temperatures at considerable depths are constant; and—in conjunction with other observations—prove that a higher temperature prevails in the interior than at the surface of the earth.

Numerous excellent experiments have been made in the different levels of many mines whilst each level had successively been the deepest;§ but it seems not to have been ascertained whether the temperature of any spot remained the same as it had been when the bottom of the mine, after other works, had been extended beneath it.

To invite the attention of others to an inquiry I am no longer able to follow, I venture to offer comparisons between the temperatures of water issuing from the deepest works on two *lodes* at *East Wheal Crofty* in 1838, and again at the same levels—somewhat extended—in 1840, when one part of the mine was twenty, and the other thirty fathoms deeper; and between the streams entering the bottom of *Wheal Vor* in 1838, and at the same depth in the present year; the works having been in the interval considerably lengthened and deepened.

East Wheal Crofty. In greenstone.	1838.‖ Depth. Fathoms.	Temp.	1840.‖ Depth. Fathoms.	Temp.
Longclose, engine lode, copper ...	85	63°·5	85	60°·
" " " ...			115	64·
Trevenson, Reeve's lode " ...	115	69·	115	62·
" " " ...			135	70·75

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* Read at the Annual Meeting of the Royal Institution of Cornwall, 13th May 1859.

† "Annales des Mines" (1^{ere} Série), III., p. 599.

‡ Cornwall Geol. Trans. III., p. 318.

§ Mr Fix, Cornwall Geol. Trans. II., pp. 14–19. *Ibid.* III., p. 313. Dr—now Sir John—Forbes, Cornwall Geol. Trans. II., p. 159. Mr Moyle, Cornwall Geol. Trans. II., p. 404. Dr Barham, Cornwall Geol. Trans. III., p. 150.

Cornwall Geol. Trans. V., p. 395.

Wheal Vor. In clay slate.		1838.*		1859.†	
		Depth. Fathoms.	Temp.	Depth. Fathoms.	Temp.
			°		°
Main lode W.	Tin ...			210	74·5
„ W.	„ ...			222	75·
„ E.	„ ...	230	78·		
„ E.	„ ...	240	80·5 81·	240	74·
„ W.	„ ...			251	80·
„ W.	„ ...			311	86·
„	„ ...			321	91·
„ Water discharged by pumps at the adit, from ...		240	69·	321	75·

Thus, on the long Engine-lode at *East Wheal Crofty*, the temperature at the bottom was 63°·5 when in 1838 that part of the mine was 85 fathoms deep; but it had fallen to 60° at that spot in 1840 when 30 fathoms deeper; yet at the bottom it was 64° when in 1840 it was 115 fathoms deep.

At Reeve's lode in the same mine it was at the bottom 69° when in 1838 that part of the mine was 115 fathoms deep; but it had fallen to 62° in that spot in 1840 when 20 fathoms deeper; yet at the bottom it was 70°·75 when in 1840 it was 135 fathoms deep.

On the Main lode at *Wheal Vor* the temperature at the bottom was 80°·5-81° when in 1838 the mine was 240 fathoms deep; but it had fallen to 74° at that level in 1859 when 81 fathoms deeper; yet it had advanced to 91° at the bottom in 1859 when 321 fathoms deep. The water discharged by the pumps at the adit in 1838, when the mine was 240 fathoms deep, was 69°; but in 1859, when the mine was 321 fathoms deep, it had advanced to 75°.

It has long been known that, independent of their conducting powers, the differing compositions and structures of various rocks, by affording greater or less facilities for the ascent of water and vapour, act no unimportant part in transferring towards the surface of the earth a portion of that heat which prevails within it; and that, modified by local influences, each formation thus possesses, as it were, its own peculiar distribution of temperature. The operations of mining open to these vehicles of internal heat freer channels of circulation than the natural joints and crevices; they thus disturb to a certain extent the normal equilibrium of temperature in their neighbourhood. As, therefore, each successive extension of deeper works intercepts the ascent of warm streams, the original bottom becomes cooler.

The foregoing observations clearly indicate as well the cooling effects of this interception, as the important differences which sometimes occur within very short distances, in rocks and veins of the same character; but they are neither numerous nor varied enough to show at what depth below the level affected the intercepting influence begins and ceases. But notwithstanding a subsequent extension of deeper works may have thus cooled the previous bottom, it still maintains a higher temperature than the mean at shallower parts of the mine: a further proof that the temperature of mines progressively increases with their depth—a general fact already sustained by irrefragible evidence from every quarter of the globe. W. J. Henwood.

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* Cornwall Geological Transactions, V., p. 394.

† For this series of observations, made by permission of my friend George Noakes, Esq., Chairman of the Wheal Vor Board of Direction—with the same instruments used in 1838—I am indebted to Captain Francis Francis.

3 Clarence Place, Penzance, 11*th May* 1859.

OBITUARIES.

Frederick Henry Alexander Baron Humboldt, the most learned man of the present age, was born at Berlin on 14th September 1760, and died on 7th May 1859. He lost his father when he was 10 years old; his mother was a cousin of the Princess Blucher. From 1787–1789 he studied at the Universities of Frankfort-on-the-Oder and Göttingen, when, among others, he had for his teachers Gottlieb Heyne and Blumenbach. During his holidays he made geological excursions in the Harz and on the banks of the Rhine, and published a work on the Basalts of the Rhine, which was the first of his publications. He early displayed a taste for travelling. On this subject he says:—"Brought up in a country which has no direct communication with the colonies of the two Indies, an inhabitant of the mountains, removed from the coasts, I felt the progressive development in me of a real passion for the sea and for long voyages. A taste for herborization, the study of geology, a rapid tour made in Holland in spring 1790, in England and in France with a celebrated man, George Forster, who had the good fortune to accompany Captain Cook in his second voyage round the world, contributed to give a determined direction to the plans for travelling which I had formed at the age of 18. It was no longer the desire of agitation and of a wandering life,—it was that of coming into immediate contact with wild nature, majestic and varied in its productions; it was the hope of finding some facts useful to the sciences which made me constantly sigh for an opportunity of visiting those beautiful regions situated in the torrid zone. My circumstances not permitting me to execute then the projects which occupied my mind, I had leisure during six years to prepare myself for the observations which I hoped to make on the new continent."

After his return from his excursion with Forster, Humboldt, destined at first for financial occupations, passed some months in the school of Busch and Ebeling at Hamburg. From June 1791, however, he attended the lectures of the celebrated Werner in the School of Mines at Freiberg, where he enjoyed the friendship of Leopold Van Buch and André de Rio. During his sojourn in Freiberg he devoted attention to the subterranean Flora, and published a work entitled, "Specimen Floræ Subterraneæ Fribergensis," which he dedicated to his master, the celebrated botanist, Willdenow. From 1792 to 1797 he occupied the position of Director-General of the mines of Anspach and Bayreuth. In 1792 the experiment of galvinism attracted his notice, and he wrote a work on the irritability of muscular fibre, &c. He was a fellow-labourer with Schiller in the periodical entitled "The Hours."

In 1796 the death of his mother seems to have excited more fully his desire to travel. He studied astronomy under the Baron Von Zach; and, after some months' sojourn in Jena and Vienna, he started with his friend Von Buch for Italy, with the view of studying the volcanoes. He was, however, prevented by the wars which prevailed at that time from accomplishing his object; and he passed the winter of 1797–98 at Salzburg and Berchtesgaden, devoting his attention to meteorology. He was invited by Lord Bristol to join an expedition which he was about to undertake to the upper parts of Egypt. He accepted the invitation, and repaired to Paris for the purpose of getting the necessary instruments, when he learned at one and the same moment, in May 1798, that Buonaparte had departed for Egypt, and that the Earl of Bristol had been arrested at Milan. He received a hearty welcome in Paris from Laplace and Berthollet, and made the acquaintance of Aimé Bonpland, his future companion in travel. He passed the winter of 1798–99 in Spain with the last-named friend. On 5th June 1799 he embarked with Bonpland in the Spanish frigate Pizzaro, and proceeded to Santa Cruz, which he reached on 19th June. The two friends ascended the Peak of Teneriffe. On 16th July they reached the Port of Cumana in South America, and during eighteen months they were employed in exploring the provinces of the State of Venezuela. In February 1800 they went to Caracas, and visited the country between the Orinoco and the Amazon. He gives a glowing description of this journey in his "Tableaux de la Nature." The basin of the Orinoco was fully explored. He traversed by river navigation 2301 geographical miles, from the Rio Negro, by the Cassiquiare river, to the Orinoco. Humboldt subsequently went to Havannah, and proceeded to Callao and Lima in Peru. At Callao he was enabled to make an important observation relative to the passage of Mercury over the sun's disk.

After two months' navigation on the Magdalena River, he visited the plateau of Bogota. Passing over the cordillera of Quindiu, the volcano of Popayan, the Paramo of Almaguer, the high plateau of Los Pastos, he reached Quito in January 1802. During five months he explored the valley of Quito, and the chain of volcanoes on the snowy summits of the Andes. He ascended Chimborazo, and reached an elevation higher than had been attained by any one, but was prevented by a crevasse from attaining the summit of the peak.

He descended by Cuença and the Cinchona forests of Loxa to the Valley of the Amazon near Jaen de Bracamoros; then traversing the plateau of Caxamarca, he reached Micuipampe and the western slope of the Cordilleras of Peru. From the Allo de Guangamarca he first had a view of the Pacific Ocean, an event which is graphically detailed in his "Tableaux." On 23d March 1803 Humboldt and his companions arrived at Acapulco after having touched at Callao and Guayaquil; they then visited the capital of Mexico and the volcano of Sorullo. After ascending the volcano of Toluca, and of Cofre de Perote, he proceeded by the oak forests of Xalapa to Vera Cruz. On 7th March 1804 he quitted the coast of Mexico and sailed for Havannah, where he spent ten months; then he embarked with Bonpland and Montufar for Philadelphia, and received at Washington a hearty welcome from Jefferson. Leaving America on 9th June, he reached Bordeaux on 3d August 1804, after having been absent from Europe for five years.

The results of this tour, so important in a geographical, ethnological, geological, and zoological point of view, have been given in his admirable works,—(1.) "Voyages aux Regions Equinoctiales du Nouveau Continent;" (2.) "Vues des Cordilleres et Monuments des Peuples Indigènes de l'Amerique;" (3.) "Recueil d'Observations de Zoologie et d'Anatomie Comparée;" (4.) "Essai Politique sur Royaume de la Nouvelle Espagne;"

(5.) "Recueil d'Observations Astronomiques;" (6.) "Physique Générale et Geolgique;" (7.) "Essai sur la Geographie des Plantes."

Humboldt may be said to have been the first who attended to this latter department of science. It is developed in his "Plantes Equinoctiales;" his "Monograph of Melastomaceæ," &c.; his "Nova Genera et Species;" his "Synopsis of the Plants of the New World," &c. All these works were published by him during his stay in Paris from 1805 to 1827.

During this time he also found leisure to attend to chemistry. He made a tour in Italy with Gay-Lussac and Von Buch, and visited Vesuvius. He accompanied his brother William in his embassy to London in 1811, and undertook several excursions in England and Germany with Arago and Valenciennes.

In 1827 he settled finally in Berlin, and became a friend of the royal family of Prussia, occupying an important position as Counsellor of State.

In 1829 he explored Central Asia with Ehrenberg and Gustavus Rose, under the auspices of the Emperor Nicolas, visiting Moscow, the Ural and Altai Mountains, and the empire of China; also visiting the steppes of Astrakan and the Caspian Sea, travelling more than 2300 geographical miles in the course of nine months. He communicated the principal results of the expedition in his "Asie Central, Recherches sur les Chaines de Montagnes et la Climatologie Comparée." This journey to Asia also enriched his "Ansihten der Natur." In this journey he completely demolished the pretended plateau of Central Asia, which had been believed by all geographers from the time of Marco Polo.

In 1843 he published his "Carte des Chaines des Montagnes et des Volcanes de l'Asie Centrale." In this map he marked the mean direction and heights of the mountains, and represented the interior of the Asiatic continent from 30° to 60° lat., between the meridians of Pekin and Cherson. The Academy of St Petersburg, as the result of this expedition, established, at the suggestion of Humboldt, magnetic and meteorological stations from St Petersburg to Pekin.

After the revolution of 1830, Humboldt was sent by Frederick-William III., as a special ambassador on the part of Prussia, to recognise Louis Philippe. He continued afterwards to pay an annual visit to Paris. His last sojourn in Paris was in 1847–48. In 1841 he went on two occasions to London with Frederick-William IV, and in 1845 he visited Copenhagen.

He who had thus about fifty years before explored the New World, and at the age of sixty visited Central Asia, now commenced, at the age of eighty, to pass in review all the knowledge which had been acquired by man relative to the heavens and the earth, in his immortal work entitled "Cosmos," the first volume of which appeared in April 1845, and the fourth at the beginning of 1858. This work contains an epitome of the physical history of the globe, and is a wonderful monument of the author's powers even at the age of ninety.

A writer in the *Daily News* makes the following remarks regarding him:—"His frame wore wonderfully; and there was no sign of decay of external sense or interior faculty while younger men were dropping into the grave completely worn out. He was the last of the contemporaries of Goethe; and as the tidings came of the death of each—philosopher, poet, statesman, or soldier—Humboldt raised his head higher, seemed to feel younger, and, as it were, proud of having outlived so many. If silent, he was kind and gentle; if talkative, he would startle his hearers with a story or scene from a Siberian steppe or a Peruvian river-side, fresh and accurate as if witnessed last year. He forgot no names or dates, any more than facts of a more interesting kind. In the street, he was known to every resident of Berlin and Potsdam, and was pointed out to all strangers as he walked, slowly and firmly, with his massive head bent a little forward, and his hand at his back holding a pamphlet. He was fond of the society of young men to the last, and was often found present at their scientific processes and meetings for experiment, and nobody present was more unpretending and gay. He has been charged with putting down all talk but his own; but this was the natural mistake of the empty-minded, who were not qualified either to listen or talk in his presence. There was no better listener than Humboldt in the presence of one who had anything worth hearing to say on any subject whatever.

"It is a great thing for Germany that, at the period when the national intellect seemed in danger of evaporating in dreams and vapours of metaphysics, Humboldt arose to connect the abstract faculty of that national mind with the material on which it ought to be employed. The rise of so great a naturalist and initiator of physical philosophy at the very crisis of the intellectual fortunes of Germany is a blessing of yet unappreciated value; unappreciated, because it is only the completion of any revolution which can reveal the whole prior need of it. If Alexander Humboldt suffered, more or less, from the infection of the national uncertainty of thought and obscurity of expression, he conferred infinitely more than he lost by giving a grasp of reality to the finest minds of his country, and opening a broad new avenue into the realm of nature to be trodden by all people of all times."

C. A. Agardh.—Carl Adolph Agardh, Professor of Botany and Rural Economy in the University of Lund, Sweden, was born at Bastud in Harland, 23d January 1785, and he died 28th January 1859. He prosecuted his studies at the University of Lund, which he entered in 1799, and devoted his attention in a special manner to natural history and mathematics. In 1807 he taught mathematics in the University. His favourite science, however, was botany. After a visit to Olaf Swartz at Stockholm, he entered upon the study of the Algæ, and in this department he acquired a well-merited reputation. He travelled on the Continent, and on his return to Lund was nominated Demonstrator of Botany. In 1812 he became Professor of Botany in the University. He now laboured assiduously at the classification of the Algæ. In his "Synopsis Algarum Scandinaviæ" he developed his system of arrangement. He subsequently published his "Species Algarum," "Icones Algarum," and lastly, his "Systema Algarum," which appeared in 1827. He published also works on the physiology and morphology of plants, as well as on systematic botany. He became a member of the Swedish Parliament in 1817, and zealously advocated all measures which tended to the public welfare. In 1816 he took orders, and in 1837 was consecrated Bishop of Carlstadt. He was much interested in education, and in the improvement of schools. He strongly urged the propriety of introducing modern science, and especially botany, into elementary education.

He did much to promote science in Sweden, and his death is a national loss. A Swedish correspondent, writing to Dr Hooker, says, "He was a singular man, in some respects a first-rate genius, but a very peculiar one. As a youth he studied mathematics, and wrote some dissertations on that subject; then hearing that botany was a very difficult science, he determined to show the little world of Lund that in a very few months he could master the science. There are probably few subjects of human knowledge on which he has not ventured to write. He was ever genial, with light, sparkling wit and good humour."

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Proceedings of the Manchester Library and Philosophical Society, March, April, and May 1859.—*From the Society.*

Proceedings of the Academy of Sciences of Philadelphia.—*From the Academy.*

The Mosaic Account of the Creation. By Dr James C. Fisher. Philadelphia, 1858.—*From the Author.*

On the Currents of the Ocean. By James D. Dana.—*From the Author.*

On Marcou's Geology of North America. By Prof. Agassiz; and Reply by Dana.—*From J. D. Dana.*

Lighthouse Illumination. By Thomas Stevenson, F.R.S.E.—*From the Author.*

Journal of the Asiatic Society of Bengal, No. IV. 1858.—*From the Editors.*

L'Institut for March, April, and May 1859.—*From the Editor.*

Memoirs of the Geological Survey of India. By Dr Thomas Oldham and Mr Harry F. Blandford.—*From the Authors.*

Map of Chicago Harbour and Bar.—*From Lieut.-Col. Graham.*

Canadian Naturalist and Geologist for February 1859.—*From the Editors.*

Meteorology in connection with Agriculture. By Prof. Joseph Henry. —*From the Smithsonian Institution.*

Catalogue of the Described Dipteræ of North America. By R. Osten Sacken.—*From the Smithsonian Institution.*

Report of the Superintendent of the United States Coast Survey for 1856. From Prof. A. D. Bache.—*From the Author.*

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Transactions of the Academy of Sciences of St Louis. February 1857 to April 1858.—*From the Academy.*

American Journal of Science and Arts. March 1859.

Jahrbuch der Kaiserlich-Königlichen Geologischen Reichsanstalt. January, February, and March 1858; April, May, June 1858.

An Examination of the Question of Anæsthesia arising on the Memorial of Charles Thomas Wells. By the Hon. Truman Smith. New York, 1858.

THE

EDINBURGH NEW

PHILOSOPHICAL JOURNAL.

The Raw Products of India suited to the Manufacturing Wants of Great Britain. Part I. By ALEX. HUNTER, M.D., F.R.C.S.E., Madras School of Industrial Arts.

Fibrous Plants.

THERE is perhaps no quarter of the globe so well adapted to supply the raw materials employed in the manufactures of Great Britain as India; whether we look to the large extent of its territory—embracing as it does so many thousands of square miles; to its varieties of temperature, elevation, climate, and soil—presenting all the characters both of tropical and temperate regions; to the geological and mineralogical features of the country; or to its teeming population, we are presented in every aspect with all the requisites for the amelioration of a country which may, by God's blessing, and by our judicious management, be made to contribute most materially to the future prosperity not only of Great Britain, but of India herself. It does not appear to be generally known how much has already been done by the late government of India, as well as by private European and native enterprise, to develop the resources of the country. On a recent tour through the manufacturing districts of England and Scotland, made by the author, for the purpose of ascertaining what raw products from Southern India were suited to the manufacturing wants of Great Britain, one of the first observations recorded was, that many of the raw products of India, some of which are already well known in the markets of Europe, might be supplied in much larger quantities, and in some instances of better quality than at present, if arrangements could be made for giving publicity in India to the requirements of our manufacturers, and if the latter could assist the natives in preparing the raw materials so as to be suited for manufacturing purposes. There is no denying the fact, that much of the raw produce of India is inferior to that of other countries, chiefly from the want of suitable machinery, skill, care, and cleanliness in preparing it for the market, or from deficient means of packing and exporting it. Our merchants and manufacturers are becoming aware of this, and efforts are being made to remedy the evil: this has been effected partially and locally, but by no means to the extent which is desirable. In describing the various raw products, we shall attempt hereafter to point out and to illustrate the kinds of machinery and the processes in use amongst the natives; and we hope that the very crudity and clumsiness of many of the means employed will do more to attract attention to the subject than any eloquent appeal or well-written description. What is most wanted, in the first instance, is, that a plain and truthful statement of facts should be laid before the public; and we have sufficient confidence in the enterprise, skill, and energy of our merchants and manufacturers to lead us to believe that they will take the safest and the most efficient steps for procuring regular supplies of those materials suited to their own immediate wants.

Cotton from India.—Government has already expended large sums of money in trying to improve and extend the culture of indigenous, American, and Bourbon cotton, and in bringing to notice the best modes of cleaning it for the European market. For many years past the benefits of this have been felt in England by an increased supply of cotton from India, and by an improvement in the quality, as well as in the modes of culture and packing. The American and Bourbon species of the cotton plant have also been extensively introduced into India, and the experiments conducted by government in the various cotton farms, established at Coimbatore and in other parts of India, have been taken advantage of, both by enterprising Europeans and natives, who are doing their best to extend the cultivation and to improve the quality of the cotton produced. It does not seem to be generally known in England, that the fluctuations in the price of cotton from India (dependent in a great measure on the supply of American cotton of better quality) have shaken the confidence of the natives in the value of this product as a safe investment for their capital. I believe it is now an established fact that, when the price of the East India cotton falls in Liverpool to 5d. per pound, it no longer proves a remunerative speculation for the merchant in England and the merchant in India who order it, the dubash or banian who collects it, the underwriters who ship and insure it, the wealthy native who sends it from up country and pays advances for its growth, and the poor ryot who cultivates it. Now, as all these parties have to get their profits out of the cotton before it comes into our factories, can we wonder that it does not always pay its expenses, or that a few handfuls of dirt frequently find their way into the bales before the cotton reaches its destination? Samples of the different cottons of America and India are being collected, along with other raw products used in the manufactories of Great Britain, and the natives will be rather surprised, in some instances, to see the different states in which their cottons come into the markets of London, Liverpool, and Glasgow, compared with the cottons of other countries. It may not be amiss to inform our men of business, that a large and promising trade in cotton is now opening up with China from some parts of Bombay and Madras, and that lands are now under cultivation in the Tinnevelly, Salem, and a few districts in southern India, for the growth of the coloured nankin, and some of the native and hybrid cottons, which are shipped to China, where they find a more steady market than in England. The natives of India are also sending their raw cottons to England to be spun into thread for their own weaving, and are turning their attention to the extended cultivation of flax, jute, hemp, and silk; and to the manufactures of sugar, indigo, and saltpetre, which appear to hold out prospects of being more remunerative than cotton. Those who are desirous of further information on the subject of the cultivation of cotton in India, would do well to consult the Reports on the Cotton Farms in Bengal and Coimbatore; the Productive Resources of India, and the Fibrous Plants of India, published by the late Dr Forbes Royle; and an admirable paper on the subject, prepared for the Royal Society of Arts, by Dr J. Forbes Watson, and published in their "Transactions," February 1859. The Manchester and Liverpool merchants, and one or two Chambers of Commerce, have been getting up an agitation on the subject of cotton from India, but they seem to be ignorant of what the government of India have already done for the development of the resources of that country. Meetings, agitations, and addresses to the Council of India, or to the Houses of Parliament, will not do much to increase the supply of cotton from India. The chief obstacles at present to the extended cultivation are, the fluctuation of the prices in the English market, from the cotton coming in competition with superior qualities from America, and the number of parties who have to be paid their profits out of it before the cotton reaches this country. If arrangements can be made to lessen this part of the heavy expenditure, and to provide for more of the profits reaching the poor ryots who cultivate the cotton, there is no doubt that the supply will keep pace with that of other products from India.

Hemp, Jute, Flax, and their substitutes.—Another large and important class of Indian raw products, suited to the manufacturing wants of Great Britain, is the Fibrous Plants. Nearly all parts of India produce fibrous plants suited for weaving, cordage, or the manufacture of paper, and for many years past these have been attracting the attention of Government and of the manufacturing communities, both in India and in England. The London Exhibition of 1851 gave a great stimulus to this branch of industry, both in Bengal and Madras, and the result has been, that materials for every description of textile manufacture, from the coarsest packing cloth to the finest cambric, have been discovered, and many of them have been proved to be well suited for manufacturing purposes. A few are already introduced into the manufactures of Great Britain, and others are steadily gaining favour in our markets for special purposes of manufacture. Among the most important of these is the Jute of Bengal, a kind of hemp which is found to be well adapted for many of our coarse fabrics. Within the last few years a large trade in this material has sprung up in Dundee, and the rapidly-increasing demand for the jute shows what the energy and perseverance of one or two practical men can accomplish when directed in the proper way. We had the privilege in October last of inspecting some of the largest manufactories in Dundee, and of meeting the principal merchants and manufacturers of that town, and of subsequently corresponding with the Chamber of Commerce through their secretary, who has kindly furnished the following statistics of the jute trade of Dundee :—

Jute imported in 1858 to United Kingdom .	37,800 tons.
Of which Dundee used . . .	30,000 „
Leaving for other places . . .	7,800 „

The quantity shipped from Calcutta to this country from 1st October 1858 (the coming in of the new crop) to 31st March 1859, amounts to fully 300,000 bales, or 40,000 tons, being the greatest quantity ever shipped. For all coarse purposes jute does better than flax or tow, and is now extensively employed in making pack-sheet, bagging, sacks, &c. The importations of it have gone on steadily increasing for the last few years, as will be seen from the accompanying tables :—

In 1838	1,136 tons.	In 1846	9,220 tons.
1842	2,740 „	1848	8,900 „
1844	5,500 „	1852	16,980 „

The following table shows the consumption of the United Kingdom, and of Dundee, for the last four years :—

United Kingdom.

	1854.	1855.	1856.	1857.	
Stocks in London and Liverpool at 1st January	7,736	11,518	8,301	8,231	tons.
Importation of United Kingdom	24,086	26,964	36,554	32,300	„
	31,872	38,482	44,855	40,531	„
Stocks in London and Liverpool at 31st December	11,518	8,301	8,231	9,457	„
	20,354	30,181	36,624	31,074	„
Consumption of United Kingdom	152,655	226,357	274,680	233,055	bales.

Dundee.

	1854.	1855.	1856.	1857.	
Imported	16,590	25,894	31,031	24,340	tons.
	124,425	194,205	232,732	182,550	bales.

The above will show how useful a fibre may become after being known ; and it may be very properly asked, why should we not be supplied with many other fibres from India ?

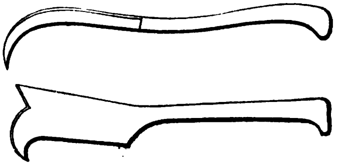
Before proceeding to consider the other fibrous materials which might be supplied from India, it may not be out of place to mention, that, after the distribution of the awards from the London Exhibition of 1851, the natives of Southern India discovered that many of their raw products from the vegetable, animal, and mineral kingdoms had attracted attention in England. With the view of keeping up the interest that had been excited amongst the natives, it was resolved by the Madras government, at the suggestion of Lord Harris, who was then the governor, to hold exhibitions of raw produce, arts, and manufactures at Madras every second year, and to have local exhibitions in most of the large districts throughout the presidency every year, at which prizes were liberally awarded. An exhibition of European and Indian arts and manufactures had been held previously in Madras, and had proved successful. The Madras Museum had also been enlarged, and was visited by great crowds of natives daily; it was therefore apparent that objects of this kind might be made beneficial both to Great Britain and to India. Four exhibitions have been held at Madras, and others at seventeen stations throughout the presidency. A number of local museums of raw produce have also been established in Southern India, and the beneficial results of this measure, suggested and carried out by Lord Harris, have already been apparent in several ways, one of which was, that while the mutinies were devastating Bengal, the Europeans, East Indians, and natives of Southern India were united in friendly harmony, aiding each other in attempting to bring to notice the resources of the country, and in supplying many of them to the Bengal government. At one of the Madras exhibitions, three steam-engines and a great deal of European machinery were exhibited at work, and lectures and demonstrations were given, usually to crowded audiences. One department that appeared to excite very great attention was the cleaning of fibres, and the following machines, which had been made up in Madras, were tried in competition to ascertain which was best suited for particular kinds of fibrous plants.

Suggestions for Cleaning Fibrous Plants for Cordage and Weaving.

Many Indian plants yield fibres well suited for cordage, weaving, and paper making. There is a considerable demand for materials of this kind in Europe, but the ordinary processes for preparing them do not yield fibres sufficiently clean or strong to pay for the expenses of freight and preparation. All steeping and rotting of plants is bad in warm climates, as putrefaction usually commences soon after fermentation, and the fibres become dirty, stiff, and harsh, or even brittle, after being long soaked. The reason of this is, that the sap of the plant becomes sour and of a dark colour within a few days, and thus destroys the fibre; the great object, therefore, is to remove the bark, sap, and all impurities from the fibres as soon as possible after the plant is cut.

Selection and Cutting of the Plants.—The best fibres are obtained from plants when in full vigour; young plants yield fine but weak fibres; old plants, coarse and often discoloured fibres. No more of a plant must be cut than can be cleaned in two days, as the sap soon dries and discolours the fibre, and the labour of cleaning is thereby increased. If a large quantity of a plant has been cut, a number of coolies must be set to clean it as soon as possible, and it should be kept in a shady place to prevent drying and discoloration. The best kinds of knives for cutting plants are of these shapes.

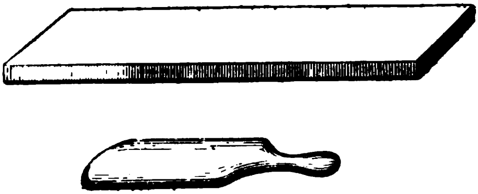
Fig. 1.



Knives for Cutting Plants.

Beating of the Cut Plants.—Plants should be well crushed, beaten, or bruised as soon as possible after they are cut. The simplest and cheapest apparatus for this purpose is a plain board and a wooden mallet, like the accompanying.

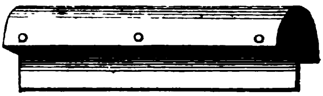
Fig. 2.



Board and Mallet.

Children may be employed to beat plants on boards till the pulp and fibre are thoroughly crushed. The plant must then be taken in small handfuls at a time, and scraped on a board. The scraper is made of hoop-iron, let into a piece of wood, and should be held in the right hand, the bundle of fibres being in the left; these must either be drawn briskly between the scraper and the board, or laid flat on the board, and the scraper drawn over them till all the pulp is removed.

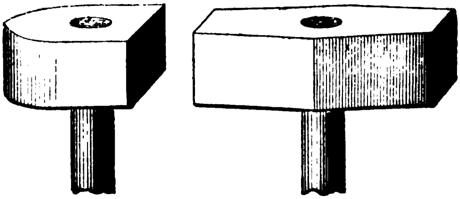
Fig. 3.



Scraper.

For bruising hard, strong plants, or barks of trees, a heavy wooden mallet, like one of the accompanying, will be required; the corners are sometimes of use for separating the bark from the wood, and as soon as this is effected, the woody part should be thrown aside, and the flat surface of the mallet used for crushing the bark.

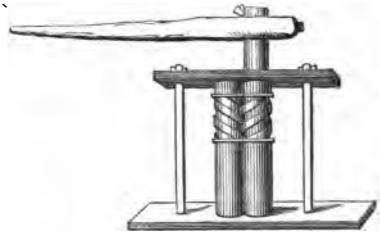
Fig. 4.



Heavy Wooden Mallets.

Another mode of crushing plants, so as to destroy the pulp, is to pass them three or four times between the wooden rollers of the common sugar-cane mill. They must then be laid upon a flat board and well scraped till the pulp is removed from the fibres. The longest fibres may then be taken in handfuls and tied into bundles, the shorter fibres being tied into separate bundles, as being of an inferior value; these bundles must be well washed in water, and allowed to soak for an hour; they are then to be washed in clean water, and hung up on ropes in the shade to dry; if they retain any green colour or impurities, these must be removed by scraping and washing. The scrapings, which contain a good deal of fibre, must be well beaten in a heap, till the loosened pulp, bark, or impurities can be thoroughly shaken out; they may then be thrown into a tub of water and well washed; if the green pulp or any impurities still adhere, they must be again well beaten on the flat board, shaken, again washed and laid out on clean cloth in the shade to dry; fibres must not be exposed to the sun until they are thoroughly cleaned, as the least remaining sap discolours or stains them. The longest and cleanest fibres are the most valuable for manufacturing purposes. The fibres obtained from the scrapings being short and entangled, can only be used as tow for packing or stuffing purposes, or as a material for paper making.

Fig. 5.

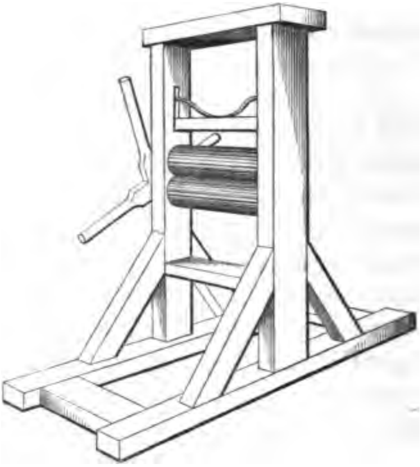


Sugar-cane Mill.

By the processes above described, which are within the means of the poorest classes, clean, strong, and pliant fibres can be produced from a great variety of plants; the processes are particularly suited to the cleaning of the Plantain, Aloe, Marool, and similar pulpy plants. To be worked economically, a good deal of the beating and crushing should be performed by children or boys, and the greatest cleanliness must be enforced, the scrapings and washings being removed every day and used as manure, the workshop floor being well washed, and the apartment freely aired and ventilated, as the effluvia from decaying vegetable matters soon become offensive, and are apt to induce a bad kind of fever. The cleaning should always be carried on in open well-paved sheds, and with a plentiful supply of water; an earthen or sandy floor soon soaks up the moist pulp, and becomes very unhealthy and offensive.

By way of economising labour, and obtaining larger quantities of fibres than can be procured by the above processes, some simple kinds of machinery have been invented, which expedite some parts of the cleaning. One of these is a crushing cylinder press, with two grooved rollers of hard wood, the upper one of which moves upon a strong steel spring, while the lower one is turned by a lever handle with four spokes. The plant to be crushed is passed three or four times between the rolling cylinders, and when thoroughly bruised, the pulp and impurities are scraped away on a flat board, as above described. The principle is much the same as that of the common sugar-cane mill, but the grooved cylinders act more effectually in crushing the plant.

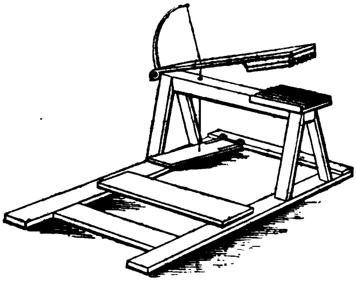
Fig. 6.



Crushing Cylinder Press.

The brake is a machine that has long been in use in European countries for cleaning flax. It consists of four pieces of hard wood, like the backs of swords, fixed on the end of a frame, which works upon another fixed frame with five similar pieces of wood; these work in the interstices of the first by means of a joint at one end. The machine is supported upon a frame-work of planks by four strong legs; there is a treadle or foot-board for working it below, and a bent steel spring above for raising the upper or striking-frame; the advantage of these is that the machine can be worked either with the hand or foot. The plant to be crushed is taken in the left hand and is placed between the two frames, the upper one being briskly brought down upon the bundle, which is moved at every blow until it is all thoroughly bruised. The plant is then passed on to be scraped, or it may be placed in water for an hour or two, till a considerable quantity has been bruised. The water is then to be wrung out of it, and the bundles again passed under the brake, when the pulp will be found to have been a good deal loosened from the fibres. The more quickly the subsequent processes of scraping and washing are performed, the whiter, stronger, and more pliant will be the fibre. On no account should the cleaning of fibres be delayed beyond the second day after the plant is cut.

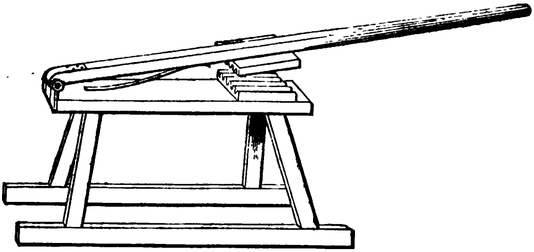
Fig. 7.



The Brake.

Various modifications of the brake have been introduced at different times. One of these, which was simple and efficient, was shown by Mr Underwood at the Exhibition of 1855; it differs from the ordinary brake in having a long lever handle and a steel spring between the hinge and the

Fig. 8.

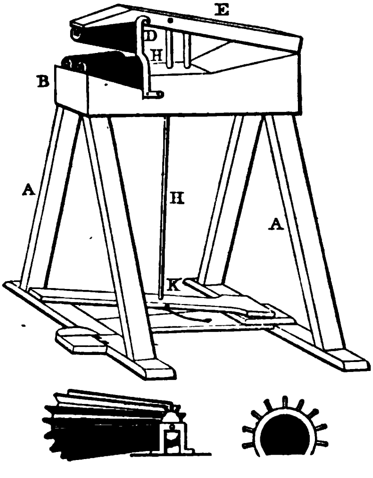


Mr Underwood's Brake.

grooved frames; the latter were covered with sheet-iron. This machine was found to be well suited for crushing the aloe, yucca, and strong, hard plants.

A machine for breaking and preparing raw flax and hemp was patented by Messrs Hill and Bundy. In this machine the frame A is made either of wood or metal, which supports two conical rollers B, these revolve independently of each other in brass bearings, a third conical roller D being similarly supported under the top piece of the machine E. These rollers may be made of hard wood or cast-iron, and the teeth must be so shaped and disposed with regard to each other as to have considerable play between them, so as to admit the plant that is to be broken and prepared. This machine is figured and described at page 221 of Dr Forbes Royle's excellent work on the Fibrous Plants of India. The upper piece of the machine E which carries the conical roller D is attached to the main frame by a moveable joint at one end, and at its other with an iron rod H, H, which is attached below to a treadle K, with a spring; motion is given with the foot, while the plant to be cleaned is held by the hands between the cones. The operation may be commenced and continued for some time with the larger part of the rollers, and finished with their smaller ends. There was a modification of this machine in the Madras Exhibition of 1857, made at the workshops Dowlaishwarum.

Fig 9.



Hill and Bundy's Fibre-Cleaning Machine.

This machine is well suited for the cleaning of flax and hemp; some of the strong Indian fibrous plants require very strong pressure or blows to crush the pulp; the grooved cylinder press seems to be the best adapted for cleaning them.

Another process, patented by Mr Olcott, consists in passing kiln-dried flax in the stem between a series of thirty pairs of long-fluted wooden rollers, which so crush and bruise the stock that most of the wood drops from the fibre, and renders the process of cleaning it easy.

It is now known that the more a fibrous plant is bruised, crushed, and knocked about while the sap is fresh in it, provided the fibres are not cut across, the more easily is it cleaned, and the softer and whiter is the fibre. Another point of great importance is that the sap should be soon separated from the fibres, as it injures both their quality and colour when it begins to decompose.

Chemical and other Processes for Cleaning Fibres.—In addition to the simple mechanical processes just described, there are a few others that might be tried in India with a prospect of success.

Watt's process consists of exposing flax to steam on perforated plates in iron chambers for twelve or eighteen hours, then passing the stalks in small parcels under two pairs of heavy rollers, by which it is pressed into flat tape-like bands and nearly deprived of its moisture; it is then hung up to dry, and afterwards cleaned by the ordinary process of scutching and heckling.

Buchanan's process consists in steeping the plants in water, kept between 160 and 180 degrees of temperature, for four hours, then drying and cleaning the fibres in the ordinary way. The reason for regulating the temperature and keeping it below the boiling point is, to avoid the coagulation of the albumen, which would prevent the thorough removal of the sap and colouring matter.

Mr Claussen's chemical process for cleaning fibres consists in boiling the cut and crushed stems of flax, hemp, or other plants, in a weak solution of caustic soda containing 1-2000th part of the alkali; the fibre is then removed to a bath of dilute sulphuric acid, containing 1-500th part of acid, in which it is boiled for an hour; it is next transferred to a solution containing 10 per cent. of carbonate of soda, left for one hour, and again transferred to an acid bath for half-an-hour, which completes the process; by this means the fibre is split up and divided in a most remarkable manner, but at present the expense of the mineral acids would prevent this process from being remunerative in India. Dr Royle, however, suggests that modifications of this method might be tried with dhobees' earth, or crude carbonate of soda and vegetable acids, as that from the *Cicer arietinum* or Bengal gram bush. Wood ashes and the bruised leaves of the tamarind might also furnish materials of a similar kind; but if processes of this kind are to be followed in India, they must be expeditiously completed, as they are apt to cause discoloration, if the plants are left in a wet state for more than two days.

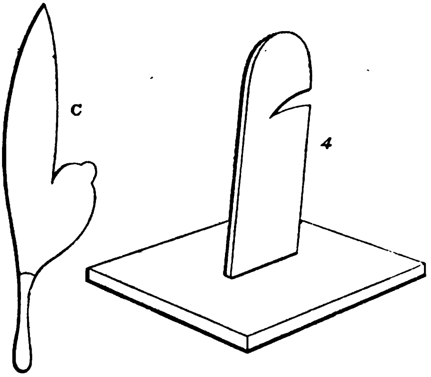
Preparing Fibres for Manufacturing Purposes.—The general complaint made in the European markets against fibrous substances sent from India is, that they are dirty, weak, and almost useless, from faulty modes of preparation and slovenly packing. In fact, they are for the most part so crude and harsh, that the bales look like so much rubbish.

Little or no trouble has been taken to render the fibrous substances with which India abounds fit for any but the commonest kinds of rope. A few bales and samples, however, that have been carefully prepared and shipped on different occasions, have satisfied the European manufacturers that India produces many useful fibres that are fit for the finest descriptions of weaving; but unless they be cleaned and prepared for the market carefully in the first instance, or while the plant is fresh and moist, no subsequent amount of labour or expense will fit them for any manufacture except paper, and even for that the Indian bales are in general so indifferent, from their dirty colour and rotten or brittle condition, that they can only be used for coarse packing-paper or pasteboard.

If the instructions which have already been given for quickly bruising, scraping, and washing away the sap and impurities from the fibres on the day when the plant is cut, be carefully attended to, every part of the plant can be made use of, and the results will be satisfactory. Thus, good, clean, strong, and pliant, or even silky-looking fibres are produced. The scrapings, if well pounded, shaken, beat, and washed, yield a white tow fit for packing purposes or paper-making. The washings and scrapings of many plants are useful for feeding animals when fresh, and they make an excellent liquid manure when sour. The wood, bark, and leaves of the plants may be used as fuel; but in this, as in other manufactures, expedition, care, and cleanliness are the great requisites. The results of instructions on this subject, given in the Jury Reports of 1855, have been most satisfactory, and some of the fibres contributed to the present Exhibition of 1857, which have been prepared according to these instructions, and with some of the machines made in the School of Arts, or from working models furnished, prove that great improvements may be made in this department of industry, and that India might be made to yield much larger supplies of fibrous substances, fit for manufacturing purposes, if a little more attention were bestowed on the early processes of preparation, and capital laid out in the erection of proper workshops and machinery.

It has been found on trial, that the same simple rules that are applicable to the cleaning of one plant, are applicable, with some modifications, to all. Barks of trees, or shrubby plants with bark, boon, or pithy stalks, require various forms of machinery to separate these. Thus, for flax, a scutching-block and wooden knife have long been in use. Fig. 10 represents a board set upright in a block of wood so as to stand steady; a horizontal slit, which is thin at the edge, is cut in it about three feet from the ground. The broken and washed flax, after being allowed to dry for an hour or two in the shade, is inserted in handfuls through this slit, so as to project to the right, and a flat wooden sword, 8 or 10 inches broad, like fig. 10, C, is used for striking the flax parallel to this board and close to the slit, so as to scrape

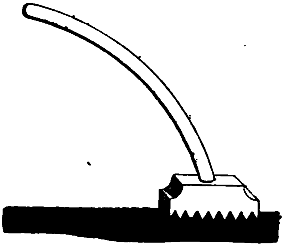
Fig. 10.



Scutching-Block and Knife.

off the boon or asperities; the part which lies in the slit is constantly changed by a turn of the left hand. It has been found by long experience that this mode of cleaning or scutching flax destroys less of the fibre than scutching-mills; the latter, however, perform the work more expeditiously.

Fig 11.



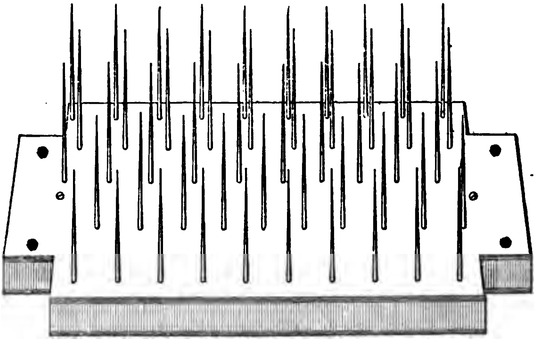
Bott-Hammer.

The bott-hammer is sometimes used for bruising or crushing fibrous plants; it consists of a wooden block, having on its under face channels or flutings which run across its surface; the block is fixed to a long belt-helve or handle, and may be worked singly, or several hammers may be worked in a row by machinery. The plants may be cleaned either in a dry or wet state; they must be laid on a clean board and beaten carefully from one end to the other; then turned and beaten again; but the whole of the impurities cannot be removed in this manner, a certain quantity of chaff still remains, and this must be removed by subsequent rubbing, beating, and heckling, when quite dry.

One of the most important parts of the process of cleaning fibres is heckling or combing them, so as to render them fit for manufacturing purposes.

The heckle is a sort of comb with several rows of strong steel teeth, fixed into blocks of wood, strengthened both above

Fig. 12.



Heckle.

and below with plates of sheet-iron or copper. The machine must be fixed with screws or bolts to a bench or strong plank. Heckles are made of different degrees of delicacy, according to the fineness of the fibre required: the teeth should be finely polished, and well tapered. The following is the mode of using the heckle:—The workman takes a bundle of fibres by the middle in his right hand, with the left he spreads them and lets them fall lightly on the points of the teeth, he then pulls the bundle towards him gently, taking care not to let the fibres sink too deeply between the teeth. He then turns the other end of the bundle and prepares it in the same way; 100 lbs. of fibre yield about 40 of tow. To assist in splitting the fibres, they may be carefully folded up into a bundle and beaten upon a block with a wooden mallet, and then well rubbed with the hands. Boiling in an alkaline ley (made of wood ashes or dhobees' earth) has the same effect. The mercantile value of fibres is much enhanced by careful cleaning and heckling.

The above suggestions for cleaning fibrous plants for cordage and weaving were prepared and illustrated in the Madras School of Industrial Arts, and widely distributed over Southern India. The experiments were commenced in 1850 in the Monegar Choultry, a poors' house in Madras, and subsequently carried on in several of the jails of Southern India. At Bangalore they were introduced by Dr Kirkpatrick into the Lunatic Asylum, and the results proved highly satisfactory, excellent rope and string having been made, which defrayed all the expenses of their manufacture. It will be seen from the suggestions, which are worthy of attention from manufacturers and machine-makers, that the processes for cleaning fibres by steeping and rotting are not suited for warm climates, as they cause discoloration and stiffness, or brittleness of the fibres; also that there is a good field for the employment of inventive skill, to devise some machines that will crush and separate the pulp or the bark from the fibre of the plants rapidly and cheaply, without injuring the fibre. Some of the machines above illustrated answered the purpose on a small scale; but if the English market is to be supplied with fibres for weaving cordage or paper-making, more powerful and efficient machinery must be supplied from England, and the chambers' of commerce and manufacturers could not perhaps render a greater boon to India than by aiding an effort of this kind, which is likely to yield a permanent benefit to both countries. Strong, thick succulent plants, like the aloe family, would probably require different machinery from the hemps, jutes, and flax plants. The barks of trees might also require to be cleaned in a different way from either.

Remarks on the principal Fibrous Plants of Southern India.

The classification of fibrous plants usually adopted by botanists is under the heads of Endogenous or inside-growing plants (characterised by the absence of bark, by having parallel veins in the leaves, and a single seed-leaf), and Exogenous or outside-growing plants (having a true bark, reticulated veins in the leaves, and two cotyledonary or seed-leaves). The Endogenous plants yielding fibres are palms, aloes, and agaves; *Yucca*, or Adam's needle; *Sanseviera zeylanica*, or marool; *Fourcroya*, or gigantic aloe; *Ananassa*, or pine-apple; *Musa*, or plantain; *Pandanus*, or screw pine; rushes, grasses, and sedges.

The Exogenous fibrous plants embrace those yielding cotton and silk cotton, flax, hemp, and their substitutes. The flax plants are the *Linum usitatissimum*, or true flax of Europe; the *Calotropis*, or yercum; *Tylophora asthmatica; Cryptostegia grandiflora*, or palay; and the *Damia extensa*, or ootrum, all yielding excellent substitutes for flax.

The Hemp plants are the *Cannabis sativa; Hibiscus cannabinus*, or ambaree; *Crotalaria juncea*, sunn hemp or junapum: *Abelmoschus esculentus*, or bendee; *Abutilon tomentosum*, or toothee; *Corchorus olitorius*, or jute; *Urtica tenacissima*, or hill nettle. In addition to these there is a large class of barks of trees yielding fibres, as varieties of *Ficus*, *Grewia*, *Bauhinia*, *Dalbergia*, *Isora*, *Butea*, *Vernonia*, *Paritium*, *Arena*, &c.

In considering the qualities of the fibrous materials produced in India, it will not be necessary to enter into lengthened details; but there are a few points deserving of attention, which it is thought may prove of special interest to the public in

Great Britain, as some of the materials to be noted are gradually finding their way into manufactures, and others would probably be employed if known.

Palms.—These yield a number of strong but coarse materials for cordage and mats, or bast; some of the fibres are already well known in commerce, as coir. This is the outer husk or covering of the fruit of the coco-nut (*Cocos nucifera*). This substance possesses several peculiar properties which have been taken advantage of by the natives; one is that it is very light and springy, hence well suited for making the running rigging of ships, and cables that allow vessels to ride at anchor in severe gales, when iron and other cables would break: it is now coming into extensive use in the navy, and in merchant and other vessels. The old dirty and clumsy process of steeping it for nearly a year in salt water, as followed on the western coast of India and on the Laccadive Islands, is now being gradually superseded, as it was proved in 1850, that by beating and washing the fibre when fresh, it might be prepared in one day, nearly white and fit for receiving dyes. Rewards having been freely offered by the Madras government for the best dyed and cleaned fibres, a considerable demand soon arose for the best qualities of coir, which were found to be suited for making fine rope, rugs, flooring-mats (both plain and dyed), brushes, and punkahs. A considerable trade in the finer qualities of coir is now springing up from Mangalore, Travancore, and some of the ports in the south and west of India. The coarse qualities of coir are largely exported from Bengal and Bombay, but they do not command so remunerative a price as the finer descriptions.

Palmyra Palm (*Borassus flabelliformis*).—The fibre of the leaf stalk is used for a coarse kind of rope for securing thatch, tying palings, for tow ropes for boats, and for raising water from wells. The leaf is also much used for making fans and large hand punkahs, and in strips for writing upon with a pointed iron stilum. This, in fact, is the material of which the books in Southern India are composed, and on which the bills and shop accounts are recorded. Experiments were made to manufacture paper from this material, but it was found to contain too much woody and dark cellular substance, and too little clean fibre; the pulp, in fact, on drying, became dark brown, short, and brittle. The ropes made from the leaf stalks are coarse and rather stiff; but it was ascertained that a good material for making baskets and bonnet frames could be prepared by passing the fibres through holes in steel plates, and washing the substance while fresh and green. Another important discovery that was made in regard to the cleaning of coir-fibres of palms and of barks having a tendency to become brown in drying was, that this may be obviated in a simple and cheap way by beating the plant when fresh, and washing it first in an alkaline ley made with wood ashes and a little quick-lime, and afterwards in plenty of fresh water. The dhobees' earth, a kind of Fuller's earth, containing carbonate of soda, also answered the same purpose if a little quick-lime was added. The only other palms in Southern India yielding useful fibres are the *Caryota urens*, a species of sago-palm, yielding a black fibre called ejoo, of great strength and of considerable length. This is employed by the natives for making fishing-lines and strong coarse nets, but it does not seem to be known in the European market. A stiffer coarse fibre resembling it, and the produce of a palm (*Attalea funiferà*), is now imported from Africa into Glasgow, and is much used for making brooms.

Elate, or *Phœnix sylvestris*, the wild date, and the *Calamus Rotang*, a wild marsh date, both yield fibres which are employed in making ropes, mats, and cables.

The leaves of the *Pandanus odoratissimus*, or screw pine, are much used in making basket-work, and plait for hats, bags, and bonnets. They also contain a fine white fibre resembling flax, but very short in the staple (not above an inch long); it was thought that this substance might be employed for weaving, but the proportion of green pulp and of prickles along the edges of the leaf, both requiring to be separated, made the process of cleaning too laborious. The fibre, however, was found to be strong, and like flax. The aerial roots of this plant are very peculiar, containing much white pulpy matter mixed with fibre of no great strength. They are employed by the natives as coarse brushes for white-washing and house painting. When the outer brown covering was removed, they were found to yield a white substance that made excellent paper, when mixed with a small proportion of rags or of old fishing-nets.

The Nars, or coarse strong fibres of India.—We come now to a most important, but as yet little known class of fibrous materials, many of which are well suited to the manufacturing wants of European countries. It will not be necessary to describe these separately, as they bear considerable resemblance to each other, though one or two possess peculiar properties; the plants which yield them are the Aloe tribe, as *Agave americana* and *A. vivipara; Fourcroya gigantea,* or gigantic aloe; *Yucca gloriosa,* Adam's needle, and *Y. aloifolia* and *Y. angustifolia; Sanseviera zeylanica,* marool or bow-string hemp. These plants all yield fibres of different thickness and length. Those of the American aloe vary from 3 to 4 feet; of the *Fourcroya,* from 6 to 9 feet, and of the *Agave vivipara,* from 3 to 5 feet. Their thickness is about that of stout pack-thread, and they are nearly white, and possess considerable strength, but a little stiffness. The fibres of the *Yuccas* and *Sanseviera* are much shorter, varying from 1 to 2 feet; they are, however, more pliant and finer, resembling human hair. Plants of this class are in general easily cleaned, and they yield a considerable proportion of fibre, but with the exception of the marool, they possess one peculiar property, which is their tendency to rot easily and rather quickly under water. On examining into the cause of this, it was found to depend upon a thick, viscid creamy juice, which can be squeezed out of the fibres when apparently clean, but cannot be got rid of by the ordinary processes of cleaning. This property also prevents ropes made of these fibres from taking tar. The simplest remedy for this defect was found to be tanning the ropes, a method much resorted to by the natives in some parts of India, and one apparently better suited than our tarring for preserving sailcloth, tarpaulin, or ropes, in a warm climate. The fibres of the aloe were extensively used at one time in India for making cordage, and for some years it was attempted to introduce this and the marool fibre for supplying ropes for the Indian navy; but the tendency to rot under water led to their being both abandoned; and the superior quality of the Manilla hemp recommended it as a useful substitute. These fibres or nars, however, are possessed of other properties which enhance their value for finer manufactures; being nearly white, they take dyes well, and are now being employed for making floor-mats and coloured rugs, damask for covering chairs, ladies' slippers, canvass for Berlin wool-work, as a substitute for willow shavings for bonnet shapes, and as a material for paper.

Plantain fibres.—A class of plants nearly allied to the aloes, and yielding a fibre that may be classed with the nars, is the Musa or plantain family. There are a good many species of the *Musa* in India, but the most common is the *M. paradisaica.* The large plantain or banana of the West Indies also thrives in many localities, and attempts are being made by the Madras Government to introduce the species that produces the Manilla hemp. There is perhaps no plant that can be so easily cleaned as the plantain; all that is necessary is to beat, scrape, and wash the stalks of the leaves and the stem soon after it is cut. The per centage of fibre is not so great as in some other plants, but from containing a little tannin, which is not removed in the ordinary process of cleaning, the fibre possesses the valuable property of resisting exposure to damp. It is now coming into use for making rope, cordage, and cloth; and we were much pleased to find that it is being employed for carriage-braid, and by Messrs Whytock and Company of Edinburgh, in making stair-rugs, and in carpets. The fibre has a very glossy surface when well cleaned, and is soft and pliant. A good instance of its durability was discovered accidentally in the Arsenal at Madras, when clearing out a go-down containing some old rope imported from Europe, and stored away some sixteen years previously; the plantain fibre from Manilla, which had been stored away among the rope at the same time, was fresh, strong, and clean, while the English rope was all rotten and like tinder, from the action of the tar and paste used in laying up the strands. It may interest our ropemakers to know, that the ordinary tarred rope of Great

Britain often becomes brittle and useless within two years when stored in a hot confined or damp locality, in a warm climate. The reason is, because the tar and paste begin to act upon each other, and to cause a sort of acid reaction that corrodes the rope. The question then comes to be, is tar as good a preservative of vegetable fibre as the process of tanning? The presumption is, that it is not, but the point cannot be satisfactorily determined without experiments in a warm climate.

The Flax of India.—The true flax, or *Linum usitatissimum,* has long been cultivated in India on account of the seed, which yields the well-known linseed oil of commerce. There having been no demand for the flax in India till within the last few years, the plant has usually been burnt in large heaps after obtaining the seed from it. The reasons which led to the burning of the crop were, that if it stands to rot or decay on the ground, it becomes poisonous for cattle; the natives, knowing that it is an exhausting crop, use it as a manure, and try to return to the soil the alkalies and salts obtained by burning the dry stalks. For some years past, attempts have been made to extend the cultivation of flax in India, and English, Irish, and Russian seed have been sent out to be cultivated in competition with Indian flax seed. The results of the experiment made in the Madras Presidency under Dr Cleghorn, the professor of botany and conservator of woods and forests, have proved that the flax plant thrives above a certain elevation, and in the cool parts of the Madras Presidency, as at Hyderabad, Mysore, the Neilgherries, Salem, the Chevaroy Hills, and in the Nagpore territories. The experiment proved a failure about Madras, and in two other hot stations below the eastern range of Ghats. Some flax grown in the Hyderabad and Nagpore territories was found, by competent judges, to be of excellent quality, and samples sent from Scinde to be tried in Dundee were pronounced by the Chamber of Commerce to be well suited for manufacturing purposes. Now that attention has been drawn to the flax of India, there is every prospect that the quantity will soon be increased, and the quality materially improved. Steps are being taken in Dundee to form a company to improve the flax trade of India, and from the success which has attended the development of the jute trade in that city, we have every reason to expect that the results will be equally satisfactory.

Indian Substitutes for Flax.—Bromelia Ananas, or *Ananassa sativa*, Pine-apple.—This plant yields a beautiful fibre for weaving, combining great strength with soft silky qualities that fit it for use as a substitute for flax. It also possesses several of the properties of true flax, being susceptible of being split by carding, and of being worked in the same way and by the same machinery; it is about the same length of staple also. At one time it was thought that this fibre could not be bleached, but it is now known from experiment that the discoloration usually found in this material is caused by a faulty mode of preparation, viz., by steeping and partial rotting; if cleaned quickly, a soft, fine, strong, white, and valuable fibre can be obtained, fit for all the purposes to which flax is applied. The plant is largely cultivated in Singapore and Manilla, where the fibre is extensively used. The species which yields the largest amount of fibre, has leaves from 4 to 5 feet in length; these are yellow in the centre, and red at the edges. This species was introduced from Manilla a few years ago by Lord Harris, and has been disseminated through the Madras Presidency.

Calotropis gigantea, or Yercum.—This is a very common plant in most parts of India, and has long been employed by the natives in making string, thread, and rope, which they consider to be the strongest they possess. The fibre was formerly employed in making cambrics and fine linens for the wealthy natives, but the tedious mode of preparing or picking it from the inner bark has always made it expensive. It is still much used for fishing-lines, nets, bow-strings, gins for catching large game, and for the Brahminical cord worn over the right shoulder by the higher classes of Hindoos. It has one peculiarity, viz., that the plant contains so much nitrogen, that it runs into a very fœtid putridity if left in water for a day or two. A milky juice abounds in the plant, which concretes, on drying, into a kind of gutta percha, but differs from it in being a conductor of electricity. This milky juice is applied when fresh to itchy eruptions, and, on drying, it coats the surface with a thin film like gutta percha. It is also occasionally used as a substitute for gum to seal letters. The root of the plant is used medicinally in cutaneous eruptions, under the name of muddar. The feathery pappus round the seed is also coming into use as a material for paper, and as a substitute for cotton in weaving. Messrs Thresher and Glennie of London have lately shown that this substance can be employed in weaving, and that it produces a soft quality of cloth; being of a silky nature, however, it does not work with the same machinery as cotton. The fibre of the bark, however, seems to be the most promising part of the plant, from its strength and resemblance to flax. Lawn and cambric, of excellent quality, were made from it by Bala Chetty, of Madras, to whom a prize was awarded at one of the exhibitions.

Cryptostegia grandiflora, or Palay, like the Calotropis, belongs to the family of the *Asclepiadaceæ*, and yields a fine fibre, similar to the last in most of its properties; also a milky juice, which concretes into a regular India-rubber, and a strong silky feathery pappus round the reed. This pappus is very brilliant, white, and satiny in lustre. Fine thread and yarn have been made from the fibre of the inner bark, and it appears to be an excellent substitute for flax.

The *Tylophora asthmatica*, or Koorinja, a medicinal plant, lately brought into notice by Dr Kirkpatrick of the Mysore Commission, contains a good deal of a strong, white fibre, possessing several of the properties of flax.

The *Damia extensa*, or Ootrum, a common weed that grows very abundantly in the Raichore, Hyderabad, and Nagpore territories, and brought into notice by Captain Meadows Taylor, also yields a fine and a strong fibre, resembling flax in several properties.

We come next to one of the largest and most extensively useful class of Indian fibrous plants, the hemps, jutes, and their substitutes:—

The *Cannabis sativa*, or Indian hemp, does not thrive so well in Southern as in Central and Northern India. The plant seems to require an elevated exposure and a cool climate; it thrives at altitudes varying from 3000 to 7000 feet, and the fibre deteriorates in the hot plains of India. The juice of the plant, however, does not lose its intoxicating property in hot localities. It is cultivated as an intoxicating plant called bhang.

Crotolaria juncea, or Sunn hemp, is known in different parts of India under the names of sunnub-vuckoonar, jute gramee or shanal. This plant yields a good fibre for twine, rope, coarse cloth, sails, and bags, also a good material for paper. It is often sold as jute, and is largely exported from India for the same manufactures.

Jutes.—Corchorus olitorius and *C. capsularis.*—The former of these plants yields one of the best qualities of Indian hemp, now largely employed in the manufactures of Liverpool and Dundee. The fibre varies in length from 8 to 10 feet; it is nearly white, of good strength, and rather glossy; it is chiefly employed in making sail-cloth, canvass, sacking and string. In Dundee there are several large manufactories, each employing from 500 to 600 people, and using only this substance, which was chiefly brought into notice by the exertions and inquiries of the late Dr F. Royle. The fibres of the *C. capsularis* are possessed of the same properties as those of the *C. olitorius.*

Besides the jute, hemp, and sunn, there are a number of other Indian fibres resembling them, and well known to the natives, who employ them for the same purposes. It will not be necessary to describe these in detail, as they resemble each other in most of their properties; the plants which yield them are—*Hibiscus cannabinus*, *H. Sabdariffa*, or roselle, *H. vesicarius*, or wild ambaree, *H. vitifolia*, *H. lampas*, *H. rosa-chinensis*, and *H. mutabilis.*

Abelmoschus esculentus, or bandikai, also known as bendee, ambaree, and vendee; *A. ficulneus*, *Abutilon tomentosum*, or toottee, *A. polyandrum*, *Althœa rosea*, Sida or Indian mallow, and *Isora corylifolia*, all yield good hemp, varying in length of fibre, as well as in strength and quality.

Nettles.—An important class of fibrous plants is the hill nettles, some of which attain a considerable size, and yield fine fibres of great strength. *Urtica nivea* and *U. pulcherrima*, *Boehmeria nivea*, or China grass, and *Girarinia Leschenaultiana*, or Neilgherry nettle, figured by Dr Wright in his *Icones Plantarum*, fig. 1976, all yield good materials for fine cloth and cordage; but as yet these, and several of the hemps and jutes, are little known in the European market.

Barks of Trees.—Many of the wild tribes in India are acquainted with the properties of the barks of the forest trees, and for centuries they have been in the habit of using some of them for making rope, string, thread, and cloth, but as yet little is known of this class of products in Europe. The following barks are known to yield fibres suited for weaving or cordage:—*Grewia asiatica* and *G. tiliæfolia*, *Malope grandiflora*, *Cordia obliqua*, *Dechachistia crotonifolia*, *Butea frondosa*, *Paritium macrophyllum*, *Sterculea*, *Urena lobata*, *Vernonia anthelmintica*, *Bauhinia*, *Ficus*, *Acacia*, *Eleodendron* and *Antiaris*, but as yet little use has been made of these except by the natives.

We have now given in a succinct form all the information that is probably requisite, regarding the fibrous products of India, to enable the public to judge whether that country may not be looked to hereafter as the source from whence Great Britain may derive a great part of the supplies requisite for the support of some of the most important branches of her manufacturing industry. The information here collected has been the result of the labours of a great many individuals, appointed by the Madras government in 1850 to inquire into the resources of Southern India. My own attention has been specially directed during that time to the improved modes of preparing the fibres for manufacturing purposes, and the results of the inquiries, instituted with the view of collecting samples and information; first for the London Exhibition of 1851, then for the Paris Exhibition, and lastly, for the three* Exhibitions in Madras, to all of which specimens of most of the fibres above noticed were contributed, have been so far satisfactory, in establishing the fact that the principles laid down for cleaning fibres are correct, and that if they be strictly followed, materials of a very superior quality may be prepared suited to a great many manufacturing purposes. During the past year I have visited several establishments in England and Scotland where fibrous materials are employed on a gigantic scale, and am much indebted for the kind and liberal way in which I have been permitted to examine machinery, and to inquire into details. In return for this kindness, I have commenced to publish such practical information regarding the raw products of India as is suited to the manufacturing wants of Great Britain. The sanction of Lord Stanley and the Indian Council was obtained for furnishing the information herein contained, and the public have the power of judging of the qualities of the fibrous materials described, by examining the specimens deposited in the museums at the India House, the Kew Gardens, and the Botanic Garden in Edinburgh.

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* A fourth Exhibition has just been held in Madras, and means are being taken by Dr J. Forbes Watson for procuring large samples for the Museum at

Another object aimed at is to attempt to enlist the sympathies of the public in Great Britain more warmly than hitherto in behalf of India and its inhabitants. The Indian government have already expended large sums in improving and developing the resources of that country, and there are many ways in which the public can confer additional benefits. Amongst these, one of the most important is to give the Fine Arts as a means of civilisation. We should be careful to give them in their purest forms, and to teach the natives the means of applying them to useful purposes, as

1. THE MEANS OF ILLUSTRATING THEIR OWN LITERATURE.
2. THE USES OF THE MICROSCOPE.
3. BOTANICAL DRAWING.

Men of science and manufacturers would materially benefit India by furnishing samples of the raw products used in manufactures, with prices attached; also, by supplying diagrams and models of simple machines for preparing the raw

the India House of some of the raw products there collected from the local Exhibitions held in the provinces during the past year.

products of India. In some instances the working machines, and the skilled mechanic to teach their use, would secure to the manufacturer regular supplies of raw products suited to his own requirements.

As a nation we owe a debt to India, and in attempting to repay it we should endeavour to do so in a way that will be acceptable and beneficial to the natives.

On the Electrical condition of the Egg of the Common Fowl.

By JOHN DAVY, M.D., F.R.S., London and Edinburgh.

Reflecting on the parts of which the egg is composed, so much resembling a galvanic arrangement, and on some well-known facts connected with the changes which occur within it, essential to its life and development, it appeared to me probable that marks of electrical action might be obtained by experimenting on it.

The first trial that I made was with a feeble galvanometer, one which I had used in researches on the torpedo, and with which I had obtained positive results; but with the egg it was otherwise; no appearance of electrical action was perceptible; the needle was not in the slightest degree affected. Other trials made with a much more delicate instrument have been not without success. These I shall briefly describe. The galvanometer employed was one belonging to Mr C. Becker of London, of the firm of Messrs Elliot and Brothers; and to this gentleman I have been indebted also for assistance in preparing the apparatus and in making the experiments. The galvanometer contained 200 yards of wire, 38th gauge. In the first trial, the terminal wires were of platinum, insulated, excepting near their points for contact, with a coating of sealing-wax. The egg, believed to be newly laid, had two small holes drilled in its shell at its smaller extremity, for the admission of the wires, and it was placed on a stand of glass.

First, contact was made by applying one wire to the outer shell, the other to the inner membrane, that next the shell, but without effect on the needle. Secondly, to the shell internally moistened with water and to the white, also without effect. Thirdly, to the white and yolk, now with some effect, especially when the wire in contact with the latter was plunged deeply into it. By changing the wires, the deflection of the needle was reversed. The trials were repeated on two other eggs, and even with results somewhat more strongly marked. In these, copper wires were substituted for those of platinum, and each wire had attached to it a platinum foil of about one quarter of an inch square, and both were insulated as before, the foil merely taking the place of the points. Now, on making the contact, plunging one wire fairly into the white, the other into the yolk, and stirring the latter gently about to secure the removal of the adhering white, such as might be attached to the foil in its passage, the deflection of the needle was to the extent of five degrees, and it was increased as to quickness of motion, and slightly, also, as to space, about one degree, by repeated contacts; and, as before, when the wires were changed, the course of the needle also was changed. With both eggs, the results differed but little. The results obtained, too, were very much the same, when the white and yolk, removed from the shell, and received into a porcelain cup, were experimented upon without being mixed. On the contrary, when the two were mixed, broken up together, no effect was perceptible on contact, except a restlessness of the needle. And the same restlessness was perceived when the contact was made with the white alone; which perhaps is no more than might be expected, keeping in mind how difficult it is to render either the white alone, and more especially the white and yolk together, perfectly homogeneous.

After witnessing these results with the galvanometer, it appeared not improbable that indications might be obtained of a current of electricity in the egg capable of producing chemical effects; and on trial it has turned out so. The apparatus used consisted of platinum foil, rather more than a quarter of an inch square, attached to platinum wires, to which were fastened fine silver wires as terminal points. The same attention was paid to insulation as in the preceding experiment. The chemical mixture used consisted of water containing a little gelatinous starch and a small quantity of iodide of potassium; a mixture which I had previously found yielded marks of the liberation of iodine when acted on by a wire of zinc and another of platinum, the other extremities of which were immersed in a solution of common salt of the specific gravity 10425. With these means, in a few minutes after the connection had been made with the white and yolk of an egg, signs of chemical action appeared; a distinct purple tint was perceptible surrounding one of the silver terminal wires, whilst the fluid round the other wire remained colourless. Further, when the wires were taken out of the mixture, the former showed a yellowish discoloration, whilst the other remained bright. Also, when platinum wires alone were used, the mixture having been rendered more sensitive of change by the addition of a few drops of muriatic acid, distinct results were obtained ; one of the terminal wires was strongly discoloured by iodine liberated, whilst the other remained free from discoloration. In the instance of the newly laid egg, the wire which displayed the effect corresponded to that attached to the copper when a single voltaic combination was used for testing the delicacy of the mixture. The contrary was seen in the instance of eggs that had been kept some time ; those operated on had been laid about three weeks, reckoning from the 23d April, and had been kept in a cool place. As to the reversal of effect just described, owing to the age of the egg, I could have wished to have seen whether it would have been indicated also by the galvanometer. I can hardly doubt about the result; but at home, being without the instrument requisite,—a galvanometer sufficiently delicate,—I have not been able to determine it experimentally.

On the results described, I shall not at present speculate, merely remarking that the agency which they indicate can hardly be inoperative in the economy of the egg in the changes so varied and wonderful to which it is subject during incubation and the growth of the chick. And the same electro-chemical action, it may be inferred, cannot but perform an important part in the ovum generally, at least in all instances in which, like the egg of the common fowl, it is composed of a white and yolk, or of substances in juxta-position of heterogeneous natures. Even in the seeds of plants, it may be conjectured, where there is any analogy of composition, it may exercise an influence.

Address delivered in the University, Edinburgh, to the Graduates in Medicine, on 1st August 1859. By JOHN GOODSIR, F.R.S., Professor of Anatomy.

GENTLEMEN,—You have now attained a position in which you are henceforward to be engaged, not only in the study of medicine, but also in the practice of it. You have become responsible for a continued course of self-improvement, and for your efficiency as physicians. Having taken your places as members of one of the three professions, the collective erudition of which constitutes the whole liberal learning of a country, you are bound deliberately to consider the nature of the position you now occupy. It devolves on me, on this occasion, briefly to indicate to you the scope and character of the duties which that position entails.

If the clerical profession demands an extent of study, and occupies a sphere of action, which bring it into relation with every department of learning, and all grades of society; if the erudition, and the knowledge of mankind, necessary for the accomplished lawyer, cannot be definitely limited, it is still more difficult to determine the line of demarcation between the province of the physician and the ever-extending area of human knowledge and activity.

The training required for any of the three liberal professions is therefore properly considered as the completion of a thorough education; and thus, those three distinct departments of professional study are, from their essential character, dependent on that general philosophical training which constitutes the fundamental object of a university.

This comprehensiveness of study, characterizing these three professions, is necessitated by the nature of their common object. Differing in importance, in accordance with their respective purposes, they have, nevertheless, this feature in common, that they have severally to do with human nature. The first has its sphere of action in the responsibilities and duties of human nature to its Maker, Preserver, and Judge; and has for its end the eternal happiness of humanity. The second is occupied with the responsibilities and duties due by members of the community to positive law emanating from supreme political authority, and has for its end the temporal happiness of humanity. The third devotes itself to the wellbeing of the corporeal frame, which, although not an essential, is nevertheless a highly important element of human happiness; inasmuch as on the condition of the body depends the due performance of the social duties of the individual and his efficiency as a member of the community.

The three liberal professions being thus directly devoted in common to the wellbeing of humanity, your share of the work is to determine the conditions on which health may be best attained, and to indicate or to supply the means thereto.

Health essentially consists in the harmonious performance of all the functions of the being. The conception of health can only be derived from our conception of life as manifested in organization. In the lowest plant, up to man himself, we unhesitatingly, and as it were instinctively, assume the health of the being as the most perfect manifestation of its life. As health is then the end to be attained by the calling of the physician, life, and more particularly life in relation to humanity, must constitute his peculiar study.

It appertains to the very essence of a liberal profession that its practice can only be finally determined when its principles have been ascertained. The principles of your profession are derived from the study of life and its conditions. Herein consists the chief difficulty with which medicine has had to contend. The very circumstance that vitality is subject to disturbance in direct proportion to the comprehensiveness of the conditions under which it is maintained, involves its study in complexities of a kind which do not oppose the advance of the science of inorganic nature. Vitality can only be investigated as it is manifested in individual organisms. Now, although the organization of the individual is a perfect system in itself, it is not the less a system dependent on conditions external to itself. All its parts and actions are prearranged in reference to as much of what is external to it as its conditions of existence involve. It can live or subsist in any locality in which these conditions are provided for it. If, again, these conditions are in any way transgressed, or if they are withheld, a diminution of health, or the access of disease, or of death, necessarily supervenes. As with the individual so with the species, the existence and health of which depend at any given time on the presence and integrity of the conditions of its collective vitality. The localization of all the various species, genera, families, and orders, of organized beings in space, and their existence or non-existence in time, are referable to this fundamental law of organization. From this law also is derived what appears to be a general principle in medicine—*that diminution of health, and the existence of disease, are the direct results of the disturbance or removal of one or more of the conditions of health*, so that the whole extended subject of the phenomena, nature, and causes of special diseases and injuries resolves itself into the investigation of the immediate or more or less remote disturbances of the conditions of health.

A second general principle in medicine follows from what has now been stated. For it appears that *the removal of disease consists essentially in the adjustment of previously altered conditions of health*, and that the part which you have to take in the recovery of those who may require your assistance is therefore altogether indirect and secondary.

If the conditions on which health and life depend were merely material,—if the forces which are at work within the organized system itself, and which associate it with the external medium in which it lives, were only such as the chemist and physicist can investigate and determine,—then the problem of health and longevity would be comparatively simple. The next difficulty in your art consists, therefore, in this, that within the living economy you have to deal with powers which cannot be measured, weighed, or subjected to calculation, but which nevertheless exercise an influence there co-ordinate with the working of its material forces. As this double sphere of action is the leading characteristic of the organized being, so it on the one hand affords the distinctive mark of organic science, and on the other constitutes the peculiar difficulty of medical art. Hence we may infer, as another general principle in medicine, that *in the treatment of disease, the adjustment may require to be, and in general must be, directed more or less as well to the psychical as to the physical conditions of the case.*

As man is distinguished from all the other organized beings in the midst of which he is placed by the comprehensiveness of the conditions of his economy, he is also peculiar in the mode in which he is enabled to provide for them. His peculiarity consists not so much in the complexity of his corporeal frame, as in the character and sphere of his consciousness. The conscious principle, if the expression may be so applied, of the horse or dog, is influenced only by external circumstances; the sphere of its activity is, so to speak, altogether external to itself; impressible from without, and therefore, in some sort, conscious of surrounding objects, it is altogether unconscious of itself. The so-called mental powers of the animal are capacities and faculties excited only by corresponding external objects, or by the recollection of these. Not endowed, therefore, with independent powers, its acts are acts predetermined for it, in the fundamental arrangement of its entire economy, with a precision, and to an extent, exactly commensurate with the conditions of its existence and welfare. The animal has consequently no field allotted to it for the exercise of judgment, and can therefore commit no error, nor be responsible for any act.

In our human economy, on the other hand, we are not only conscious of the material objects which surround us, but we have, in addition, a consciousness, even more vivid, of our conscious principle itself. We recognise in our economy, moreover, not only certain capacities and faculties, the proper ends, operations, and scope of which are directly predetermined and arranged, as in the lower animals, for certain essential requirements; but we are conscious, in addition, of beliefs, capacities, and faculties, the objects of which are indicated, and their operations conditioned and regulated, by the laws of the conscious principle itself. In virtue of the endowments of this, his higher principle, man is enabled to extend continuously his knowledge of the laws of external nature, and his influence over her. From the same source he derives his consciousness of the law of duty, and of that liberty of action with which it is associated: hence also, through free knowledge and moral liberty, the unassisted human reason acquires the conviction of a supreme lawgiver.

You will now observe why it is that man is distinguished from the lower animals by the comprehensiveness of the conditions of his economy. In the case of the lower animal, the means by which the conditions of the welfare of its economy are secured and adhered to are provided in its instincts. Although man, again, has also had secured to him, through his instincts, certain essential conditions of his economy, nevertheless the general conditions of his wellbeing, under the ever varying circumstances in which he is placed, are—irrespective of revealed truth—only indirectly provided for him, through his self-conscious intelligence. His instincts, in common with his corporeal frame, constitute an organism, and so far the human constitution is similar to that of the lower animal; but the organism in man is merely the instrument of his self-conscious intelligence, and it is this circumstance which entails upon him the comprehensiveness of the conditions of his welfare. He commences life less amply provided with instinctive securities than the lower animal. He must acquire the use even of his organs of sense, and of his limbs, by a self-conscious process of experiment. The knowledge of external objects, which is gradually accumulated, and the control over them which is acquired by the individual during his life, and by the species collectively, is the gradual result of a continuous struggle between his conscious principle and that material nature by which it is surrounded and penetrated; and for this continuous effort his organism is the instrument. In like manner, the due performance of all his duties, personal and social—his duties to his Maker, his duties to his fellow-men—is, from the very constitution of his conscious intelligence, a life-long struggle between truth and error, fulfilment and non-fulfilment. These collective peculiarities of the self-conscious principle, as contrasted with the instinctive manifestations of the organism, constitute the proper personality of man, as distinguished from the mere individuality of the lower animal.

Such are the comprehensive conditions of the welfare of the human economy. Their extent depends upon the endowments of the human conscious principle. Now, as the most remarkable of these endowments are the capacity of discriminating, and the liberty of choice between truth and error—between right and wrong—there exists a constant liability to disturbance. The disturbance is not so great, nor are its consequences so detrimental, in the progress of science as in the sphere of duty; for, as the acquisition of knowledge by intellectual effort is precisely conditioned by the laws of our consciousness itself, and the motives to the application of it to economical purposes sufficiently powerful, the obstacles to the progress of science are continuously diminishing. In the sphere of duty, again, the disturbing element—the tendency to select the wrong instead of the right—is in constant operation. It is not necessarily affected by the progress of science and its economic applications. On the contrary, the occasions for its disturbing action would appear to become even more numerous as so-called civilization advances.

It is sufficient for the sequence of my argument that at this point I merely allude to that Dispensation which provides the aid necessary for man in the sphere of his duties and responsibilities—that Dispensation, the nature and application of which constitute the object and calling of another profession.

The number of injuries and diseases which occur in man is much greater than in any of the lower animals. The conditions of the welfare of the latter are strictly limited to the cosmical arrangements of their special areas of distribution, while their instinctive endowments determine precisely the amount of disturbance of health, or the amount of death, which occasional or periodic cosmical changes produce. So also injury and loss of life are necessary conditions of the general organic economy. For the life of a carnivorous animal involves the death of the animal on which it feeds, as the life of the herbivorous animal involves the death of the vegetable. Domesticated animals are liable to numerous diseases and special injuries; but these are due to their association with man, who entails upon them much suffering, from which they would be saved if left to the guidance of their own instincts. As disease, then, is the result of a divergence from the conditions of health; as man is privileged, in virtue of his conscious intelligence, to provide for himself the conditions of health over the extended area of the globe, and under a never-ceasing variation of circumstances; but as he is, at the same time, liable from the nature of his conscious intelligence to diverge from those principles of truth which guide to the knowledge of the conditions of health, and to neglect that sense of duty which indicates the proper application of that knowledge when acquired, he becomes subjected to the necessary evil consequences. These consequences I need not enlarge upon. They involve all the disease and suffering which result from the neglect or infringement of duty to ourselves and to our fellow-men. They stand related to all the questions of personal and social ethics, and all the demands of public hygiene. Finally, they constitute the grounds of another general principle in the philosophy of medicine, which is, *that the greater liability of man to disease is intimately related to his higher conscious intelligence.*

How essential, then, gentlemen, must it be in your profession that you should possess a clear and comprehensive conception of all the arrangements by which human life is conditioned and modified. How vague and limited are our conceptions of these arrangements apt to be. We are apt to look for them in the dissecting-room and pathological theatre, and to forget that their most influential elements are beyond the reach of the knife, or the penetration of the microscope. Even when compelled to take into consideration the relations of the conscious intelligence to the bodily frame, we are apt to consider it as an intrusion into a department of inquiry which may adjoin, but which forms no part of our own. I venture to insist upon this topic, because by some it may be considered as entirely foreign to medical interest; and by others as involving questions admitting only of metaphysical discussion. But the reciprocal influences of the conscious and material elements of the human constitution must be admitted as all-important conditions of health and disease, and the investigation of the laws of these two opposite influences demands only a rigid adherence to both of the distinct methods of inquiry respectively peculiar to psychical and to physical science. And, moreover, this is not the question as to whether the body is only a form of the mind, or the mind a product of the body. It is not the question as to whether the mind is merely deposited in the body, or whether the mind accumulates and arranges the different parts of its own habitation, and regulates and controls them during life. These are questions interesting in the history of philosophy, and involve metaphysical discussion properly so called; but they are questions having no immediate bearing on our topic, which includes an extended series of facts, intimately and immediately connected with the wellbeing of humanity.

Every decided advance in philosophy or science is coincident with the ingress of clearer conceptions of the object to be attained, and of the method of attaining it. Towards the acquisition, therefore, of a clearer conception of our subject, it is very important, not only that the distinctive characters of the conscious principle and of the material frame should be kept steadily in view; but also that the two perfectly distinct methods of investigating them should be rigorously adhered to. Now, we find, in the present phase of our professional science, that although the rigid application of the precise methods of chemical and physical research to the investigation of the organic structures and actions has proved that the influence of chemical and physical force extends far beyond the limits formerly assigned to it in the living economy, nevertheless this has in no degree weakened the evidence of a coexisting element in organization neither chemical nor physical. For in proportion as the test-glass, the galvanometer, and the kymograph, transfer successive departments of organic science into the domains of chemistry and physics, so much the more remarkable do the characteristics of organic chemical action and of anatomical configuration become, and all the more striking and peculiar are the phenomena of consciousness felt to be.

The characteristic peculiarity of chemical action in the organism appears to consist in this, that certain of its products are such as are never met with in inorganic nature. It would appear as if chemical force in the organism were under the control of an influence which, while it confines that force in the greater part of its function to a special form of action, does not thereby render it less a chemical force than when it acts in inorganic nature.

In like manner, while the different forms in which physical force exhibits itself in the several domains of inorganic nature are exhibited in the corresponding domains of the living being, it nevertheless appears, in certain of its most important departments, to be confined by some influence to a manifestation of itself, such as it never exhibits beyond the limits of organization.

As, however, some of the most striking features of organic form have now at last been reduced to geometrical characters, and subjected to mathematical analysis, there appears to be no ground left for the assumption that all organic forms and movements are not immediately or directly due to physical forces, or do not admit of being investigated and determined by the sole application of physico-mathematical methods.

If, then, gentlemen, I have exhibited correctly the present position of certain important departments of medical science, what are its future prospects? It will in the first place, undoubtedly, as its several departments merge into the exact sciences, assume gradually a more precise character, and demand from its cultivators, and from those who may desire to enter within its precincts, a much more thorough physico-mathematical training than has hitherto been considered necessary to the science or art of medicine.

In the second place, as every advance in chemico-physical truth is followed sooner or later by a corresponding application of it to the wants of humanity, so we may confidently look forward to a continuous increase in the number of chemico-physical appliances to the amelioration of human suffering, and to the prolongation of human life.

Again, as it must be admitted that the science of organization, and more particularly the entire science of the human economy, necessarily involve the laws of the instinctive manifestations and of the conscious intelligence; as, moreover, it is essential to every increase in the clearness of our conceptions of these laws that they should be investigated through the only medium which our human, and therefore limited faculties supply; and as the advance of chemico-physical science into the domains of organization has only had the effect of bringing the instinctive manifestations and the fundamental facts of conscious intelligence which are involved in organization more strongly and distinctly into view, and of reserving them for the methods proper for their investigation, we may, I believe, confidently anticipate great progress in the psychological department of organic science.

As we have already seen that the peculiar liability of man to corporeal injury and to disease is directly related to the intellectual and moral departments of his constitution, we may confidently assume that the more careful study of these departments of the human constitution, in their relations to disease, will tend greatly to the amelioration of human suffering and to the longevity of the race. And here I would observe, that in as far as disease is mediately dependent on dereliction or neglect of personal duty, in so far also as it depends on social dereliction and neglect of duty—in all these relations of disease, you, as physicians, are only indirectly interested. It would be a great mistake, however, were you to assume that you have fulfilled all the duties of your calling when you have treated the cases which come under your observation. Your science can alone supply information regarding the primary causes of prevailing disease, necessary for the selection of the proper public measures for its prevention. I need here only remind you how much has already been done, and is now doing, in this direction, and express my belief, that as the greatest boon which your profession has hitherto conferred on the community has been the prevention of disease, her future services in the same direction will not be less valuable.

Throughout this address I have insisted much on the intimate relation which exists between the conscious principle in man and his liability to disease; and I may therefore here remind you how much the comfort of the patient, and the satisfactory progress of his cure, are dependent on the character and demeanour of the physician. If the psychical condition of your patient undoubtedly influences his corporeal state, it becomes an essential part of your duty to secure for yourselves that respect and confidence which are so readily accorded to your profession, and which tend so essentially to the welfare of those entrusted to its care. This character and this demeanour will be best secured by looking on your profession not as a mere science, with its formal application, but as an extended series of duties, the nature and scope of which are indicated in the very nature of the profession itself.

In conclusion, permit me to say, that if a tendency produced by my own studies, and if what I hold to be the fundamental principles of the special science which I profess, have given a peculiar colouring to this address, or have tempted me to allude to topics which might appear out of place, or might require more delicate handling than I can give them, my excuse is, that in the present crisis of this University, and in the fulfilment of the special duty which devolves upon me on the present occasion, I felt myself called upon to define explicitly, from my own point of view, the present positions and relations of medical science and of your profession.

For my colleagues and for myself, permit me also to state, that in dissolving the tie between us as teachers and pupils, we welcome you most sincerely as Graduates of this University, and members of our common profession.

The Mosaic Account of the Creation. By JAMES C. FISHER, M.D., Member of the Academy of Natural Sciences. Communicated by the Author.

The substance of the following paper was originally given as a verbal communication, at the meeting of the Academy of Natural Sciences, on the 9th of May 1854, in reply to the strictures of W. Parker Foulke, Esq., on the lecture of the late Hugh Miller, "The Two Records—the Mosaic and the Geologic." It was the design of the author to show, that Mr Miller, so far from using the classification by geologists, of the rocks on the earth's surface into three great groups, the "*palæozoic, secondary, tertiary,*" to illustrate the striking coincidence between the two records, in an unauthorized manner, was perfectly justified in showing that this classification, *made without any reference to Scriptures whatever*, yet did in a most wonderful manner agree with them. He endeavoured to show that, by taking the most prominent fact in each of these periods, Mr Miller had only followed the course which Moses had taken with each of the other so-called days. He had not stated, and did not intend to state, that these *were the only facts*, but that in each of them they were the most prominent and characteristic. Circumstances at the time prevented the author from writing out his remarks for publication with those of Mr Foulke, and no good opportunity occurred until the present summer, when they were published in the form now given in the "Presbyterian Quarterly Review." It has been a source of regret to the author that they were not published at the time, as they would probably have saved the lamented Mr Miller from the feeling expressed in the notes to his last work, "The Testimony of the Rocks," in regard to the remarks made by Mr Foulke, which were certainly made in no unkind spirit towards Mr Miller; for any such feeling was at the time most explicitly disclaimed.

The various methods by which theologians and geologists have sought to reconcile "The Testimony of the Rocks," and our version of the first chapter of Genesis, may all be reduced to two, or perhaps three, general schemes. The first one supposes that between the first verse and the second there was an undefined and enormous interval of time, in which the various geological changes, such as we now find upon the earth, took place; that the earth was then brought into the chaotic state described in the second verse; and then it was, in six days of twenty-four hours each, prepared for the habitation of man, who was at that time placed upon it. This was the plan of reconciliation of Dr Chalmers, and, with a single exception, that of Dr John Pye Smith, who thought that the chaos described in the second verse, and the work of creation in the rest of the chapter, extended over but a small part of the earth's surface, and that outside of that area the rest of the earth continued to enjoy the light of the sun, and plants and animals lived, and grew, and have continued by an unbroken series of generations to our own times. The progress of geological discovery has caused the scheme of Dr Chalmers to be laid aside, for it does not meet the wants of the case; and that of Dr Smith is opposed to the record of Moses, in making no provision for the creation of the heavens.

The second method supposes, that the days were periods of great and indefinite extent, each embracing vast ages, in which the various geological changes occurred. With some few modifications, this is now adopted by the great majority of modern geologists. There is little, if any, doubt that so far at least as the length of the days is concerned, this scheme is strictly in consonance with the meaning of the Scriptures. Almost all geologists and theologians, however, commit the mistake of confining this description of the creation to the earth alone, although the sacred narrative as plainly asserts that "in the beginning God created the *heavens* and the earth," and at its close declares, "thus the *heavens* and the earth were finished, and all the host of them."

Professor Barrows, in commenting upon this word, says, that "Tuch remarks, that this is the only passage in which the word *hosts* includes earthly objects along with the heavenly host. It denotes the orderly marshalling and arranging of all created things in heaven and earth." We have a right, then, to require that any system of interpretation which shall be presented to us for adoption shall account for the heavenly bodies as well as the earth; and it will not do, as we shall soon see, to confine the sole description of their creation to the work of the fourth day. Such an interpretation must not only accord with geology, but likewise with astronomy. It must, in short, be so read as to give us an account of the creation of the heavens as well as of the earth.

Before proceeding to examine and determine the meaning of the Mosaic record, we may premise that that interpretation which, fairly made according to those rules by which we interpret all language, shall best harmonize with all the facts, is most likely to be the true one, even though it may be very different from the one which we have been accustomed to regard as correct. If it best agrees with all the phenomena, we ought not to reject it on account of novelty, and assume that it cannot be true, because so many learned and wise scholars, on whose opinions we have been accustomed to rely, have given a different reading. It may be, that they have never examined it from the right point of view to attain the knowledge of its meaning.

We will now proceed with our undertaking. Verse 1st:—"In the beginning God created the heavens and the earth." Professor Lewis has employed a large part of the sixth chapter of his "Six Days of Creation," in proving that the word translated *create* does not mean to bring into existence from nothing, but rather to arrange matter previously existing. It seems, however, more reasonable to think that it was the design of Moses to teach, in opposition to those who believed in and taught the eternity of matter, that it was created by the power of God. In fact, the absolutely literal translation of the verse conveys exactly this idea.

In our version, the particle אֵת, which means *the substance of*, is not translated; were it rendered, the verse would read thus:—"In the beginning God created the substance of the heavens and the earth." The authorities for this reading are many and important. Dr Wilson, in his Easy Introduction to the Knowledge of Hebrew without the Points," in a note on this word, says: "This particle following an active verb, and going before a noun which has the servile ה prefixed, admits of no translation unless we render it 'the substance of.' Here the sense will allow it, which is rarely the case." So Harris, in his "Pre-Adamite Earth," in a note on this first verse, says: "According to the Rabbins, the verse should be rendered, 'God in the beginning created the substance of the heavens and the substance of the earth.' They understand אֵת here to mean the substance or material. The Syriac translation gives the same sense. Compare Gesenius on this word; Aben Ezra; Kimchi, in his "Book of Roots," and Buxtorf's "Talmudic Lexicon."

The adoption of this reading throws light upon the subsequent verse, and assists us to understand more clearly its meaning.

Verse 2d:—"And the earth was without form and void, and darkness was upon the face of the deep, and the Spirit of God moved upon the face of the waters."

It has been held that the particle translated *and* in this verse does not necessarily imply a direct connection between this verse and the first, and that an immense period of time may have elapsed between them. Barrows and others have, however, shown conclusively that this is erroneous, and that it has here its proper power as a direct copulative. This is also evident from the verse itself. What is the object of this verse? Is it not to describe the condition or state of the substance of the heavens and the earth, the creation of which has just been affirmed? Professor Lewis says, "'Without form and void' are expressions, the one referring to utter irregularity of dimensions and outward extent, the other to the deficiency of gravity; denoting not so much an absolute as a relative want of weight; in other words, a fluid or rarified condition, with an absence of all cohesion or solidity, or, it may be, a huge nebulosity," &c.; and again, "The תְהוֹם, or *deep*, is evidently the תֹהוּ, *without form*, mentioned before. It is etymologically different; and yet the word, as here used, can be only another name for the chaos, though afterwards employed to denote other objects which the imagination might regard as presenting some resemblance to the primeval waste." The word *waters*, in this verse, is also used to designate the same as the deep. We would here also remark, that the word מְרַחֶפֶת, rendered *moved upon*, is in the Hiphil conjugation, and is therefore causative, and would be more properly rendered, *caused motion in*. The phrase, *the face of*, is idiomatical, and answers to our word *throughout*. We can now understand the meaning of the verse. Moses is describing, in his masterly manner, by a few bold expressions, the appearance of the matter of whose creation he had just spoken. It was formless and void, or filling all space, and without any cohesion or solidity; it was all dark; and a motion caused by the Spirit of God pervaded it. The Creator now proceeds to form this formless and void matter into those bodies which He had from eternity designed. The first act was the endowing a part of this dark matter with luminous properties. Verse 3d :—"And God said, Let there be light, and there was light." The language used does not imply a new creation of matter, but simply giving to matter already created luminous power.

Verse 4th :—"And God saw the light that it was good, and God divided between the light and the darkness." The expression, God saw that it was good, does not imply moral goodness, but that it was fitted for the designed end, the purpose for which He formed it. This remark applies also to each place in the chapter in which the expression occurs. The word here rendered *divided* expresses a gradual act, such as the separation of two dissimilar substances would be. How this separation was finally effected we shall presently see.

Verse 5th :—"And God called the light day, and the darkness He called night; and the evening and the morning were the first day," or literally, "evening was, morning was, one day." The name *day* is here used evidently in a different sense, in the first part of the verse, from what it is in the last. In the first part, it undoubtedly is a name given to the light to designate its special character. Gesenius and others derive it from a root which signifies to be warm, hot, to glow with heat, and therefore its signification as a name will be, that which produces heat, or the warmth-producer,—a name which fairly expresses its principal character, and is in this respect like our word *caloric*, with which it seems to be identical in meaning; so also the term *night* is here used, not to designate a portion of time, but as the name of the dark or non-luminous matter from which the luminous had been, in this work of the first day, separated. It is, says Wilson, derived from the root לוּל, signifying to turn to, or towards; to move around; and as a name would be, the moving around matter.

In the latter part of the verse, the term *day* means a period of time. The true meaning of this word here has been one of the chief difficulties in the way of the interpretation of this chapter. Many have contended that it means, in this place, a period of twenty-four hours, or what we call a natural day; and their main argument has been the reference to the work of creation in the fourth commandment. They contend that God, in the reason which is there given for hallowing the seventh day, settles this point, that the days of creation were natural days. Now, there is no fact more evident than that the word *day* is used in the Scriptures in a variety of senses, one of which we have in the first part of this verse, where it certainly has no reference whatever to time or duration. When it does mean duration or time, it is by no means restricted to the meaning contended for: on the contrary, it has so many different ones that we can only determine it from the context. The instances of these are numerous. In the next chapter, we are told in the fourth verse, "These are the generations of the heavens and the earth,—in the day that the Lord God made the earth and the heavens." Here the term *day* includes the whole six days of the creation. So, when Job says, "Turn from him that he may accomplish, as an hireling, his day," he uses it to express the lifetime of a man. When our Saviour said to the Jews, "Abraham rejoiced to see My day," He used it to designate the period of His appearance upon earth. We have also the prophetic use of the word for a year, and many other uses of the same character, so that we can only determine the meaning of the word from the context. Professor Lewis says the Hebrews use the word יום, *day,* "for any period of time presenting a complete course or unity of events, irrespective of precise duration. There can be no doubt at all of such usage." We would reply to the argument for the limitation of time in the fourth commandment, that we are told in the next chapter, that God rested from His work on the seventh day, and blessed the seventh day and sanctified it. Now, we wish those who contend for this limitation of the *six* days to tell us when the seventh day ended, and when God ceased to rest from His work. The term *Sabbath* is also used to signify a rest of more than a natural day. It is so used in the Levitical law to designate the Sabbath of the land, or every seventh year, and in other places. The meaning of the word *day* is unquestionably limited by the context, and in each subsequent passage, to the series of completed events with which it is connected. Here the context limits it to the period from the creation of matter to the separation of light matter from the dark matter; and as no sun was yet in existence, it could not have been a day measured by it.

Verses 6th, 7th, and 8th. "And God said let there be a firmament in the midst of the waters, and let it divide the waters from the waters. And God made the firmament, and and divided the waters which were under the firmament, from the waters which were above the firmament, and it was so. and God called the firmament, heaven: and the evening and the morning were the second day." There is no part of the account of the creation that has more puzzled commentators.

Perhaps it is not possible to find any exposition of this work of the second day that has yet been given, that, when fairly examined, does not involve a downright absurdity. We will mention two examples of these; one given by Cruden, the author of the Concordance, as the understanding of divines in regard to it in the year 1737, and the other by Prof. Barrows, of Andover, in the year 1806. "The word used," says Cruden, "is רָקִיעַ, which is translated expansion, something expanded, or firmament, something firm and solid. By this word the Hebrews understood the heavens, which, like a solid and immense arch (though it be soft and liquid), served as a bank and barrier between the upper and lower waters; and that the stars were set in this arch like so many precious stones in gold and silver, when firmament is taken for the starry heaven; then, by upper waters, is meant that sea or collection of waters placed by God above all the visible heavens, and there reserved for ends known to Himself. If by firmament we understand the air—called the expansion, because it is extended far and wide, and the firmament, because it is fixed in its proper place, from whence it cannot be moved unless by force—then by superior waters are to be understood the waters in the clouds; and these may be said to be above the firmament of air, because they are above a considerable part of it."

Professor Barrows of Andover says, "In this azure vault (the sky) God has placed the heavenly bodies; the fowls fly above the earth on its face: that is, along under it, as if skimming its surface, and it constitutes a permanent division between the waters above and below itself. The waters under the firmament are those on the earth's surface. The waters above the firmament are not directly the clouds, but rather that invisible store-house of waters whence the clouds are, from age to age, supplied. Such seems to be the representation of the sacred writer. And now, what is there in this at which modern science can justly take offence? Is it that he describes the firmament as an outspread vault, in which are placed the sun, moon, and stars? Is it that he places an unexhaustible reservoir of water above our heads? That God has such a reservoir there is certain; for He has been pouring down rain from it for six thousand years, and it is not yet spent!" Certainly this is almost equal to the child's idea of the sky; "A great blue curtain drawn overhead, with holes in it to let the glory of heaven through." A very beautiful idea for a child. We answer the professor's question seriously, in the words of Hugh Miller—"that philology cannot be sound which would commit the Scriptures to a science that cannot be true."

The difficulty arises here from an entire mistake as to the meaning of רָקִיעַ, and the waters here mentioned. The word is derived from a root which means to expand, to spread abroad, and, as a noun, it may be rendered expansion. Now, what is the meaning here of expansion? Is it not a division of the formless and space-filling mass into different parts, and by an interval or expansion that can be measured from one to another? In other words, the matter of the universe was now divided into all those parts which, by their consolidation on the succeeding day, were to form not only our earth, but all the heavenly bodies. This gives us an intelligent idea of what the work of the second day was. It was the division of the matter formed in the beginning, and on the first day divided into two great classes, the light and the dark, into those innumerable parts which were to form the heavens and the heaven of heavens.

Verses 9th, 10th, 11th, 12th, and 13th. "And God said, Let the waters under the heaven be gathered together into one place, and let the dry appear, and it was so; and God called the dry earth; and the gathering together of the waters called he seas; and God saw that it was good. And God said, Let the earth bring forth grass, the herb yielding seed, and the fruit-tree yielding fruit after his kind, whose seed is in itself upon the earth; and it was so. And the earth brought forth grass, and the herb yielding seed after his kind, and the tree yielding fruit, whose seed is in itself, after his kind; and God saw that it was good. And the evening and the morning were the third day." The work of the third day was, first—the consolidation of the matter of the universe here designated as "the waters under the heavens." Throughout all the regions of space this work of consolidation went on simultaneously. Previous to this third day of creation, no geological changes could have taken place, for the earth had no separate existence. Now, however, they commence, and, as the earth becomes fitted for the existence of life upon it, it is supplied. The second part of the work of this day was the clothing the earth with verdure by the creation of plants in rich abundance; the operations of this day and the fifth are consecutive, for the work of the fourth day extended over a part of each of these days. The third, fifth, and sixth days are the only ones with which geology has anything to do, and, for the manner in which the two records agree, we must refer to the late work of the lamented Miller, "The Testimony of the Rocks," especially to the lecture—The two Records, Mosaic and Geological.

Verses 14th, 15th, 16th, 17th, 18th, and 19th: "And God said, Let there be lights in the firmament of the heaven, to divide the day from the night; and let them be for signs and for seasons, and for days and for years. And let them be for lights in the firmament of the heaven to give light upon the earth; and it was so. And God made two great lights; the greater light to rule the day, and the lesser light to rule the night; he made the stars also. And God set them in the firmament of the heaven to give light upon the earth, and to rule over the day and over the night, and to divide the light from the darkness; and God saw that it was good. And the evening and the morning were the fourth day."

It has puzzled many to know why the sun, moon, and stars were not said to be made before the fourth day. If the reader has followed carefully the course of interpretation, he can now see why they are not mentioned before. The word here rendered *made* is not the one which is rendered *create*, but one which most frequently means constituted, appointed, or set in order. The work, then, of the fourth day was the ordering and arrangement of the motions of the heavenly bodies; and their functions, so far as our earth is concerned, are clearly stated. The undoubted object of this was to guard men against making them objects of divine worship; they were created things, the work of the Deity; and, so far as man was concerned, they were designed to serve his convenience and promote his welfare. Let us now recapitulate the work of the several days, and see how they agree with the teachings of the works of God.

In the beginning God created the substance of the heavens and the earth, and this substance was without form and void, or diffused throughout space, it was dark, and the Spirit of God caused a motion to commence in it. God endued a part of it with luminous properties, and a part He left dark; He then caused the light to separate from the dark matter, and named the light matter day, or the warmth-producing matter; and the dark He called night, or the moving-around matter. This constituted the first day. On the second day, He caused the matter of the heavens and the earth, or of the universe, to separate and divide into distinct masses; and to the space which contained these masses, together with the masses themselves, He gave the name of heaven. This was the work of the second day. On the third day, He caused the masses of matter to become consolidated, and gave to the one which we inhabit the specific name of earth, and to its collections of waters, seas. He then clothed the earth abundantly with verdure of all kinds, and commenced its preparation for the residence of man upon it; this was the work of the third day. On the fourth day, He arranged the motions of the heavenly bodies, both with reference to the earth and to each other. On the fifth and sixth days, the preparation of the earth for the residence of man was completed, and man was placed upon it. We have thus a clear, definite, and intelligible narrative, which agrees throughout with the teachings of the most perfect science. We have not space now to review the various phenomena of nature which bear us out in the assertion; but those who have studied the subject will understand the full force of the declaration, that if one should seek to give a sketch in the fewest words of the Celestial Mechanism of Laplace, the Cosmos of Humboldt, and the geology of the latest and best authorities, he would do it in the very language of Moses. Here, then, we have presented to us the wonderful spectacle of all the grandest conclusions of science epitomized, arranged, and accounted for ages ago, at a time when we are accustomed to look upon the world as in its infancy, and when all nations, except the one to which this wonderful writer belonged, were plunged in the darkest and most degrading idolatry. Where did Moses get this knowledge so absolutely perfect? Was it not from God? and is not this chapter, over which such a premature shout of triumph has been sent up, the most convincing proof of the inspiration of the Scriptures? And so it will ever be, no matter what assaults may be made upon it, whether it be in regard to the unity of the race, or some other which shall yet be brought forward, all will prove in the end vain and futile, and the Scriptures will come out of the contest like the three Jews from Nebuchadnezzar's fiery furnace, without even the smell of fire having passed upon them.

On the Diurnal Oscillations of the Barometer. By Thomas Davies. With a Plate.

In glancing over Humboldt's "Cosmos," the writer had his attention called to the diurnal oscillations of the barometer, when a hypothesis suggested itself which appeared satisfactorily to account for the phenomenon.

On examination as to what had been done towards the solution of this problem, he could discover no trace of a similar hypothesis until after this paper had been quite completed and written out. He then, however, found, in the British Association Transactions for 1840, the abstract of a paper by Mr Espy, who assumes the same primary cause of the phenomenon as that propounded in this paper; but there are important differences in detail, sufficient, the author thinks, to make the proposed hypothesis essentially new. These differences cannot properly be taken up till after the full consideration of the subject in this paper, consequently they are reserved for an appendix.

Besides this hypothesis, the author had previously found three others by which the phenomenon was attempted to be explained.

The first of these is that of Professor Kæmtz, as stated in his "Course of Meteorology." He supposes a meridian of heated air, corresponding nearly with the mid-day meridian, expanding and rising above the level of the colder portions of the atmosphere. This crest of expanded air is supposed to overflow eastward and westward, so as to fill up the depression caused by the contraction of the colder portions of the atmosphere where the influence of the sun is less, or is entirely withdrawn. The flowing off of the top of the crest is supposed to lessen the weight of the atmospheric column at that point, which is indicated by a fall of the barometer, and this is supposed to correspond with the 4 o'clock P.M. minimum, which occurs shortly after the hottest part of the day.

Then the overflow in each direction eastward and westward from this crest, being an addition to the amount of air in the adjoining regions, is supposed to give a greater weight of atmosphere there, and thus to account for the two maxima of barometrical pressure which take place, the one about seven hours before, and the other about six hours after, the minimum just mentioned; but he does not satisfactorily account for the other minimum, or show why the two branches of overflow should not meet at the point of greatest depression, forming only one maximum of pressure there, as is supposed in General Sabine's hypothesis.

But there is another objection to this hypothesis. Is there sufficient ground for supposing that there is really such an overflow, except perhaps to an extent almost, if not quite, inappreciable?

Whatever be the actual height of the atmosphere, $\frac{98}{100}$ths of it (by weight) are contained within about a height of twenty-five miles, and it will be near enough for our present purpose to consider this as practically the limit of our atmosphere. Suppose an extreme of say 20° Fahr. between the maximum and minimum temperature of day and night (taking the case about the equator), we will get, roughly speaking, a difference of four per cent. between the height of the atmospheric column at these points,—that is, a difference of about *one mile.* Now the distance between these points, taking the average of the two distances eastward and westward, is the semi-circumference of the earth, or about 12,000 miles. Consequently the slope on either side of the meridional crest will have an average inclination of 1 in 12,000, down which the upper portion of the crest is supposed to flow.

Let us for a moment look at the belt or zone of heated air at the equator, forming there an equatorial crest, having its corresponding depressions of cold atmosphere about the north and the south poles. We may safely, I suppose, assume that the difference between the polar and equatorial temperatures of the atmosphere* is not less than four times greater than the difference between the extreme day and night temperature about the equator; consequently, so far as this question is concerned, we may assume that the difference between the crest and the greatest depression is four times greater in the one case than in the other, but the distance from the equatorial crest to the polar depression is only one quarter of the circumference of the globe, or half the distance in the former case; hence, roughly speaking, we may consider the average inclination of the slopes of the equatorial crest to be eight times greater than that of the slopes on either side of the meridional crest, and thus the force tending to produce currents of air in the latter case should be only about one-eighth of the force in the former case.

But this is not all. The heated equatorial crest is stationary, or nearly so, and the currents produced by it are the result of a force acting continually at the same point and in the same direction. The heated meridional crest, on the other hand, moves rapidly over the surface of the earth, so that before it has had time to establish a decided current at any point in one direction, say eastward, the crest has passed this point, and the forces are acting on the same portions of air in the opposite direction.*

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* The difference between the mean temperature of the equator and of the north pole is about 80° Fahr.

Taking, then, these two circumstances into consideration, namely, the comparatively weak influence of the meridional crest tending to produce currents of overflow from the warm to the cold regions of the atmosphere, and that even this small influence is almost, if not altogether, neutralized by the alternation of the direction of the force, we cannot consider that there is practically any such overflow as is required by this hypothesis.

We have seen, then, the difficulty, even on the assumption of an overflow, of accounting for the double maxima and minima, and also the great improbability of there being such a thing as an overflow, at least to any appreciable extent; there seems, therefore, sufficient reason for considering that this hypothesis is not the proper solution of the problem.

The second hypothesis is that of General Sabine.† He assumes, as in the previous hypothesis, an overflow of heated air, but he only supposes one minimum of gaseous pressure at the heated crest, and one maximum at the depression of greatest cold. He however brings into play the tension of the vapour contained in the atmosphere also having only one maximum and one minimum in the course of the day, but not corresponding with those of the gaseous pressure of the atmosphere. The supposed pressure from these two sources he combines so as to produce two maxima and two minima with such ingenuity, that one almost feels reluctant to attempt to controvert the hypothesis.

However, if the objections to the influence of the supposed overflow in the former hypothesis be considered valid, they will be equally so in the present case. And as the influence of the vapour, according to this hypothesis, cannot of itself account for the phenomenon, but requires the combined influence of an overflow, the whole hypothesis must fall to the ground.

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* To illustrate the effect of an alternating direction of force, let us suppose a flat, smooth board, say two feet long, and that the one end is elevated above the other by a quarter of an inch, a boy's marble placed upon it will begin to roll down the inclined plane, and will acquire a decided velocity before it reaches the lower edge. But suppose the marble placed in the middle, and that each extremity is alternately raised while the other is depressed, the amount of motion of the marble will be less and less as the rapidity of the alternating motion of the board is increased, until the marble will become apparently stationary.

† See British Association Transactions, 1844, or Philosophical Magazine, 1845, vol. xxvi.

But there are various objections that might be urged against the supposed influence of the vapour of the atmosphere in producing the phenomenon under consideration; this, however, may suffice. The diurnal oscillations of the barometer occur with remarkable regularity, there being a slight difference in amount, according as the weather is clear or cloudy. On the other hand, the variations in the humidity of the atmosphere are very great, and, especially in temperate climates, very irregular, occurring frequently at very short intervals, and often irrespective of the time of the day. It seems, therefore, very improbable that a phenomenon of so great regularity should be accounted for by a cause so irregular.

Although, then, the influence of vapour may account more or less for the irregular oscillations of the barometer, and may have probably an influence to some extent of a regular kind on the diurnal oscillations, we cannot look to it for the solution of the problem in question.

The other hypothesis is that of Mr Hopkins.* He accounts for the forenoon maximum of pressure by the weight of vapour added to the atmosphere by the sun's heat from sunrise till about nine o'clock. He accounts for the afternoon minimum partly by calling in the aid of a supposed overflow, as in both of the previous hypotheses, and partly by a supposed process of condensation of the vapour rising upward with the heated air to higher regions of the atmosphere, and there forming clouds. He says, "The vapour in the upper part of the air, being thus removed by conversion into water, no longer presses as vapour, or with the same force as that below, and the lower vapour consequently rises more freely to the height of the cloud," &c. Now, if the vapour is removed as such, its weight as water is still included in that of the atmosphere. Then its condensation, by removing its pressure as vapour on the lower vapour, so that this rises more freely, and consequently allows evaporation at the earth's surface to proceed more rapidly, causes a more rapid addition to the total amount of moisture in the atmosphere, and thus to the whole weight, so that one would from this expect rather an increase of weight than a diminution.

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* See Philosophical Magazine, 1846, vol. xxviii.

The writer has to confess that he has great difficulty in distinctly realizing the supposed influence of this formation of cloud in producing the minimum to be accounted for, and therefore he may not be doing justice to the theory in the previous remarks; but there is this objection to the theory, that the oscillations occur whether there are clouds in the upper regions of the atmosphere or no, while both of the objections to the previous hypothesis apply equally to this.

BEFORE stating the hypothesis which the writer has to propose, it will be as well to state the facts for which it has to account, as graphically given by Humboldt in his "Cosmos:"

"*Variations of atmospheric pressure*, to which belong the horary oscillations, occurring with such regularity in the tropics, where they produce a kind of ebb and flow in the atmosphere, which cannot be ascribed to the action of the moon, and which differs so considerably, according to geographical latitude, the seasons of the year, and the elevations above the level of the sea."

"The horary oscillations of the barometer, which in the tropics present two maxima, viz. at 9 or 9¼ A.M., and 10½ or 10¾ P.M., and two minima at 4 or 4¼ P.M. and 4 A.M., occurring, therefore, in almost the hottest and coldest hours, have long been the object of my most careful diurnal and nocturnal observations. Their regularity is so great, that in the day-time especially, the hour may be ascertained from the height of the mercurial column without error on the average of more than fifteen or seventeen minutes. In the torrid zones of the New Continent, on the coasts as well as at elevations of nearly 13,000 feet above the level of the sea, where the mean temperature falls to 44·6°, I have found the regularity of the ebb and flow of the ærial ocean undisturbed by storms, hurricanes, rain, and earthquakes. The amount of the daily oscillation diminishes from 1·32 to 0·18 French lines from the equator to 70° north latitude, where Bravais made very accurate observations at Bozekop. The supposition that much nearer the pole the height of the barometer is really less at 10 A.M. than at 4 P.M., and, consequently, that the maximum and minimum influences are inverted, is not confirmed by Parry's observations at Port Bowen (73° 14′)."

The following is a translation of that portion of the descriptive letter-press of the German Atlas to the "Cosmos," which refers to this subject:—

"The regular daily oscillations of the column of mercury, or the atmospheric ebb and flow, was pointed out by Alexander von Humboldt. In Germany, the barometer rises from early morning till the forenoon, then it sinks till the afternoon; again it rises till the evening, to sink anew during the night, when by next morning it reaches its original position, supposing in the meantime no irregular variation to have taken place. Besides these times of maxima and minima of the height of the barometer, which Von Humboldt calls horary oscillations, there is a small variation according to the season. In summer, the position of the first three extremes removes further from mid-day than in winter, and the smaller minimum is usually that in the afternoon, the greater minimum, on the contrary, being that in the morning. Again, the amount of oscillation, or the difference between two consecutive extremes, is not the same at all seasons nor in every state of the weather, but in summer and in clear weather it is greater, while in winter and in cloudy weather the observations indicate a sluggishness of motion.

"The heating power of the sun has the greatest influence on, if it is not the only cause of, the daily oscillations, for they are modified by the days and seasons, while their amount depends upon the latitude."

Having the facts before us, let us now look into the question.

The extreme regularity of these oscillations, with their universal prevalence in all localities, at all elevations, during all seasons, and in all states of the atmosphere, seem to point to a simple and not to a complex cause for the solution of the phenomenon. Then, as stated by Humboldt, there seems little doubt that the heat of the sun is the principal, if not the sole, cause of this phenomenon; and Professor Kæmtz says, "It is probable that the phenomenon is due to the calorific action of the sun; Bouguer suspected it, and Laplace and Ramond have admitted this explanation." We should therefore look for a solution of the problem in some direct and simple influence of heat, rather than in its more indirect influences. Let us then examine the effect of the sun's heat on a column of air of the height of our atmosphere, assuming it about the torrid zone, where the heat is greatest, and the days and nights are nearly equal.

Without taking into consideration the ascending, descending, and horizontal currents produced by the heating effect of the sun's rays, nor the weight of vapour absorbed by the heated air, nor its pressure from tension at different temperatures, nor the electrical phenomena which may be the ultimate result of this mingling of currents and absorption of vapour, there is one simple fact that may be stated concerning the column of the atmosphere under consideration as the effect of the sun's heat, namely, that its average temperature is greater at the heat of the day than at the coldest period of the night.

But the effect of this increase of temperature is to produce on the column of air an effort to expand. It cannot expand laterally, for it is surrounded by similar columns wanting similar accommodation, consequently it must expand upwards, and the result will be that the column will be considerably higher during the day than during the night.

Now this column, although it occupies a different bulk at each of these different times, and is consequently of different specific gravity, is, on the whole, of the same weight, and, consequently, in so far as this is concerned, no change will be indicated by the barometer.

During this process of expansion upwards, the centre of gravity of the column has been raised a certain amount further from the surface of the earth, which is equivalent to the whole weight of the column being elevated by that amount. But it requires a force to put this weight in motion and to elevate it through this height. This force is produced by the expansive effort of the column of air itself acting against the surface of the earth, and raising itself up in a manner somewhat analogous to the case of a man who, stretching himself upwards, raises himself on his toes, and the result will be, that while the expansive force is in operation, there will be a pressure arising from it in addition to the pressure due to the mere weight of the column of air, and which will be indicated by a rise in the barometer above the mean height.

This process of expansion takes place from about sunrise till some time after mid-day, when the maximum heat of the atmosphere is reached. The maximum amount of pressure arising from this expansive force will, it is clear, not take place at its very commencement, as the increase of heat and consequent expansive force is then small; neither will it take place when the air has reached its highest temperature, for before that time the upheaved column of air has attained its greatest upward velocity, while the *increase* of the expansive force has ceased. But the maximum pressure will take place at that point where the *rate of increase* of expansive force has the greatest ratio as compared with the upward velocity attained at the time by the column of air.

Again, the momentum attained by the upheaved column will tend to carry it upwards after the extra pressure has been removed; but as by this time it has reached its maximum temperature, it begins to lose its heat, and the expansive force begins to *decrease*. In consequence, the surface of the earth will be relieved of a certain amount of pressure, which pressure will reach its minimum at a point where the rate of decrease of expansive power by the loss of temperature has the greatest ratio, as compared with the velocity of the fall of the column of air after it has reached its highest point of elevation above the earth's surface, and has begun to descend.

The time of greatest heat of this column of air may be taken at about two hours or more after mid-day. We have seen that the point of minimum pressure of the atmosphere should take place some time after this, and by observation it generally occurs about 4 o'clock P.M.

From this time, being late in the afternoon, the expansive force of the column of air quickly decreases till some time after sunset, when the rate of decrease becomes less for the rest of the night. In the meantime, the column of air descends with increasing momentum; while, having ceased to contract with sufficient rapidity, so as to make room for its continued fall, it will consequently be compressed to some extent by its own falling weight, and a corresponding pressure will be communicated to the surface of the earth, which will be indicated by a rise in the barometer. By observation, this pressure attains its maximum generally about 10½ o'clock P.M.

By the time the momentum of the falling atmosphere has been neutralized, an amount of compression and corresponding pressure will have been produced inconsistent with a state of equilibrium, and consequently there will commence a resilient action, until the atmosphere, rebounding from the earth's surface, again passes the point consistent with a state of equilibrium, and again reaches a point of maximum elevation, where, in consequence, there will be a second minimum of pressure, as indicated by the barometer, and which occurs about 4 o'clock A.M. This minimum, however, being only the result of a rebounding force, should not be so small as the former, that is, should not deviate so much from the mean pressure, and this agrees with observation.

Were the sun not to return for some time, so as not to interfere with the temperature of the air, we should have a series of oscillations and consequent maxima and minima occurring at about equal intervals, diminishing, however, in amount until equilibrium was gradually established. We would consequently have another point of maximum pressure some time in the forenoon, irrespective of the sun's renewed influence. But by this time the sun has again begun its heating influence, and the column of air beginning again to expand, the expansive elevating force rises, if we may so speak, to meet the falling column of air. In consequence, not only is the time of maximum pressure thus somewhat anticipated, so that this last interval is the least of all the intervals between the extremes, but this last maximum is the greater of the two maxima of the day. By observation, it takes place generally about 9 o'clock A.M.

After this, the united resilient and expansive forces will again produce an upheaving of the atmosphere, with a succession of maxima and minima similar to those of the preceding day.

Professor Kæmtz gives in a table the hourly variations of the barometer for a number of places in different latitudes. The variations for places about the torrid zone being greatest, thus admitting of greater accuracy of observation, while at the same time they are most equable, are best suited for showing the law which these oscillations follow. For this purpose, the tabular results for the four places about the torrid zone, given by Professor Kæmtz, are represented in Plate X., Diagram 1.

It would have been desirable to have had the hourly variations of temperature for some of these places, or even of some other place about the same latitude, for the purpose of comparing them with the oscillations of the barometer; but as the writer had not such information, he took that which he thought would suit best for giving an approximative comparison.

Professor Kæmtz, in another set of tables, gives the hourly variations for each month of the year for four places. The most southerly of these places is Padua, in latitude 45° 24′ north. At the equinoxes, the lengths of day and night at this place correspond most nearly with those of the torrid zone. The actual amount of heat being greater at the autumnal than at the vernal equinox, the daily observations of temperature at the former season will probably correspond most nearly with those of the torrid zone. Consequently the author was led to adopt the daily observations of temperature at Padua for the month of September as the nearest approximative representation he had within his reach of the daily variations of temperature within the tropics. This curve is shown in Diagram 2. Of course it is to be kept in mind that it is not supposed that this curve represents even approximately the *actual amount* of temperature, nor even the *actual differences* of temperature, but merely, approximately, the *comparative rates of increase and decrease* of temperature at the various hours of the day, which is sufficient for our present purpose.

On examining this curve of temperature, it will be seen that the rate of increase is greatest from about 7 to 9 or 9½ o'clock A.M., being at the rate of 1·24° Cent. or 2·23° Fahr. per hour,* while after that this rate of increase gradually lessens till about half-past 2 o'clock P.M., when the maximum temperature is reached. Now the end of this greatest rate of increase of temperature,—that is, the point where the rate of increase of the expansive force begins to abate,—corresponds as closely as possible with the time of maximum pressure, as shown by three of the curves of Diagram I., namely 9 and 9½ o'clock A.M.; while the minimum pressure at 4 o'clock P.M. occurs an hour and a-half after the maximum of temperature.

Again, the greatest rate of decrease of temperature occurs in the afternoon between 4 and 6½ o'clock, being then at the rate of 0°·96 Cent., or 1°·73 Fahr. per hour; while from 7 P.M. till 5 A.M. the rate of decrease, while nearly uniform, is only 0°·36 Cent., or 0°·65 Fahr. It is during this time of comparatively equable temperature that the maximum and minimum of least variation from the mean pressure occur; the former, as already stated, about 10½ P.M., and the latter about 4 A.M.

AN illustration suggests itself to the writer which may assist in realizing more distinctly the process supposed to go on in the atmosphere according to the preceding hypothesis.

Suppose the elastic column of air represented by a carriage on very elastic springs. Suppose the motion of this column of air relative to the sun to be represented by the motion of the carriage along a road. When the column of air approaches towards and passes under the sun, it is expanded, and consequently elevated; and let us suppose that the elevating force, instead of taking place in the carriage itself, is produced by some sudden elevation in the road. As the carriage is drawn along the level road, the pressure upon the road is uniform, being equivalent to the actual weight of the carriage; but as soon as the sudden elevation in the road is met with, there is an increase of pressure produced upon the road against the side of the elevation facing the approaching carriage. In consequence, the carriage is jolted upwards, but at the same time the elevation is passed, so that there is a decrease or minimum of pressure on the other side of the elevation towards the retiring carriage. Having thus received a jolt, the carriage, in its subsequent course over the level road, will continue for a short time to undulate, thus producing a series of maxima and minima of pressure upon the road; these, however, gradually diminishing in amount until they become inappreciable.

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* According to the table in Kæmtz, the difference between 8 and 9 o'clock is 1°·72 Cent. And this would make the correspondence with the maximum of barometrical oscillation more decided, but there seems to be a mistake in the table here, perhaps of 0°·50 Cent. (See correction in diagram.) This and the other supposed mistakes, corrected in the diagrams by dotted lines, the author has less reluctance in supposing, from the circumstance of his having discovered several of a similar kind, of some of which there can be little or no doubt. They are much more apparent on a more distorted scale than that shown.

Suppose, now, that instead of one elevation on a level road, there were a succession of elevations occurring at intervals, so as nearly to coincide with every alternate undulation of the carriage, we would then have a continued series of two maxima and two minima of pressure on the road for each elevation passed; the maximum and the minimum of greatest deviation from mean pressure being on either side of the summit of the elevation, while the maximum and the minimum of least deviation occur over the interval or level portion of road between the elevations. These maxima and minima correspond with those of the pressure of the atmosphere, as indicated by the daily oscillations of the barometer.

Diagram III. has been prepared to represent this process to the eye. The thermometric curve of Diagram II. has been laid down so as to represent in section a portion of a road, the elevations in the road corresponding with the elevations of temperature, each in their respective cases being considered as the elevating forces. The body of the carriage is represented by a ball resting on a spiral spring, so as to allow of the indication of a great amount of oscillation, while, the spring being fixed to the frame and wheels, the whole is supposed to be passing rapidly along in the direction of the arrows.

The top of the ball is represented as following a curve (indicated by the dotted line), the reverse of the barometric curve for La Guyara, the curve being set off from the thermometric curve or line of supposed surface of road. The barometric curve has been shown reversed, in order that the maximum pressure might be indicated by a compression of the spring, and *vice versa*. The barometric curve of La Guyara has been chosen in preference to the others, because it seemed on the whole to be the least exceptional in its appearance, while at the same time there is one small irregularity about 7 and 8 P.M., which nearly coincides with a similar irregularity in the thermometric curve; thus helping us to see how the one irregularity in the barometric oscillations may be affected by an irregularity in the temperature. The irregularities in the two curves just alluded to, though nearly, do not exactly coincide; but in the thermometric curve or line of surface of road, as shown in this diagram, the irregularity has been slightly shifted backwards, so as to correspond better with that in the barometric curve, the amount of alteration being indicated by the dotted line. Of course it is to be kept in mind that the *amount of barometric pressure* is not measured by the *amount of compression* of the supposed spring, but that the *differences of barometric pressure* are indicated by the *differences of compression* of the spring.

THE intervals between the maxima and minima already mentioned may be taken as the average for tropical countries; but we may readily imagine that local influences, such as the position of oceans, continents, deserts, and mountains, may interfere in producing a difference in this respect, and also in giving different values to the maxima and minima for different localities, which, however, seem to be very regular for each particular place.

This hypothesis readily accounts for the other facts, as stated by Humboldt—namely, that the difference between the maxima and minima is not so great in winter and in cloudy weather as in summer and in clear weather; for in winter the expansion of the whole atmosphere, and in cloudy weather the expansion of the lower strata, will be less than at other times. Again, the oscillations are not so great in the higher latitudes, where the influence of the sun's rays is not so great, and is perhaps more unequal according to the altitude. Then at great elevations the depth and weight of atmosphere being considerably diminished, will account for the difference in amount between the extremes there and at a lower level. It also accounts for the circumstance, that in Germany (and doubtless in other places where the difference in the length of the day is considerable, according to the season) the position of three of the extremes removes further from mid-day in summer than in winter. Thus, on account of the earlier rising of the sun, the expansion will commence sooner, and consequently the forenoon maximum will occur sooner; then the longer time of subjection to the heating influence of the sun, and the point of maximum temperature being later in the day, there will be a longer interval of oscillation, and consequently we have the afternoon minimum and evening maximum later.

HITHERTO the atmospheric column has been spoken of as a whole in reference to its temperature, expansion, and oscillatory motion, and this has been sufficient for the purpose of a general description of the hypothesis; but for the purpose of calculation, it would be necessary to consider the atmospheric column as divided into a number of strata of equal weight—the temperature, and consequent expansive force, the actual amount of expansion, the amount and velocity of motion, and consequent momentum of each stratum, requiring to be taken into consideration.

As illustrative of what is required, and at the same time to give distinctness to our conceptions of the supposed motions of the atmospheric column, the following facts may be stated.

Roughly, a column of air, one foot square, of the height of our atmosphere, weighs one ton.

If we suppose this column divided into ten strata of equal weight, each will weigh about 2 cwt.

At a uniform temperature of 60° Fahr., the lowest stratum will occupy a height of about half a mile from the earth's surface. The five lower strata (or the half of the column by weight) will occupy a height of about $3\frac{1}{4}$ miles. Nine of these strata will reach to a height of about 12 miles; while the whole ten strata may reach to a height of 50 or 100 miles.

Suppose now an extreme difference of say 20° Fahr. between the night and the day temperature of each of these strata, we would thereby have, roughly, a difference of 4 per cent. in the bulk of each. The upper strata, at the same time that they themselves expanded, being upheaved by the expansion of the strata below, there would be a difference of 4 per cent. between the night and the day level of each.

Thus, while the centre of gravity of the lowest stratum, which is about $\frac{1}{4}$th of a mile high, would only rise through $\frac{1}{100}$th of a mile, or between 50 and 60 feet, the centre of gravity of the sixth stratum (at a height of about 4 miles) would be raised through about $\frac{1}{6}$th of a mile; that of the ninth stratum, whose height may be taken at 10 miles, will be elevated through $\frac{4}{10}$ths of a mile; and that of the tenth stratum, which may be taken at a height of 18 miles, will be elevated $\frac{3}{4}$ths of a mile. That is, over a surface of one foot square, we have ten masses of matter, each weighing about 2 cwt., raised upwards distances varying from 50 or 60 feet to about $\frac{3}{4}$ths of a mile.

But we have only considered the amount of motion or displacement due to expansion. There will be also a certain amount of motion due to momentum, after the elevating force has ceased to act, just as the undulating motion of the carriage is greater than the inequalities on the road. The amount due to this we do not at present attempt to estimate; but we may perhaps safely say, that about the tropics we have over each square foot ten masses of matter, each weighing about 2 cwt., bobbing up and down twice a-day through heights varying from about 60 feet to 1 mile.

At first sight, the mode of speaking of the upper strata being upheaved by the expansion of the lower strata may be objected to, on the ground that the actual process in nature is, for the lower portion of the atmosphere in immediate contact with the earth to get more rapidly heated, and to rise upwards, being at the same time replaced by a falling colder portion of air.

No doubt there is this mingling of the air to a certain extent, although, in the opinion of Professor Kæmtz, its influence is not great in affecting the temperature of the upper portions of the atmosphere; but whether this mingling be great or small, it does not affect the question at present under consideration. The column of atmosphere has been supposed divided into strata of equal weight; these strata are considered as occupying different elevations at different times, according to their temperature. But this is quite irrespective of the internal motions which may have taken place between the different strata; for it is clear that, so far as the present question is concerned, any effect produced on a particular stratum by the addition of a portion of heated air rising into it from a lower stratum, will be counterbalanced by the equivalent amount of air that has left the stratum to supply the place of the other; and it would be quite the same to suppose no displacement of particles to have taken place, provided there is taken into account an addition to its temperature corresponding to that brought by the newly-entered portions of heated air.

THIS, then, is the hypothesis now proposed as a solution of the phenomenon of the diurnal oscillation of the barometer. There may be other slightly-modifying regular influences which must not be overlooked in a close consideration of the question. There is, for instance, the influence of the alternating heat and cold of day and night on the moisture of the air, which, however, I have no doubt, is much smaller than that attributed to it by General Sabine; there is also the tendency of the two atmospheric waves of this hypothesis, which pass round the earth in the course of every twenty-four hours, to produce a certain amount of horizontal displacement, giving another set of two maxima and minima in the course of the day, although, from the proportionably small height of the waves, the effect is probably quite inappreciable; and perhaps there may be other influences.

The cause now adduced seems, however, to be that which has decidedly the greatest influence in producing the phenomenon under consideration, and especially in producing that peculiarity of it—namely, the double oscillation in the course of the day. It seems to account in a simple and satisfactory way for all the general phenomena of these diurnal oscillations; and the writer thinks that the same hypothesis could readily account for some of the anomalies that occur in particular localities, but in the meantime he does not consider it expedient to speculate on these, especially with insufficient data.

Appendix.

As already stated, the preceding paper had been quite completed and written out without the author having been able to discover any trace of a hypothesis similar to that here proposed. Since then, however, he has found in the "British Association Transactions" for 1840 the abstract of a paper by Mr Espy, in which a similar hypothesis is propounded. Although both hypotheses agree in so far as they refer the phenomenon to the same primary cause, there are important differences in detail.

Thus, while Mr Espy agrees with the hypothesis of the preceding paper in attributing the evening maximum to the influence of the momentum acquired by the falling atmosphere, he does not take into account this influence in assisting to produce the forenoon maximum. Then he leaves out of consideration altogether the influence of the upward momentum acquired by the upheaving of the atmosphere in producing both the afternoon and morning minima;—thus he considers the afternoon minimum as *solely* due to the diminished tension of the atmosphere produced by its rapid diminution of temperature, while the morning minimum he considers to be simply the return of the atmosphere to its *mean pressure*.

Now, on the examination of these diurnal oscillations, especially as shown by diagrams, it seems evident that no hypothesis can be correct which makes the *morning minimum* correspond with the *mean barometric pressure;* and this of itself may have been considered so serious an objection to Mr

Espy's hypothesis as to have prevented that attention to the primary cause assigned by him which the writer thinks it deserved.

On the Genus Galago, with description of an apparently New Species (Galago Murinus) *from Old Calabar.* By ANDREW MURRAY. *With a Plate.*

In the last *envoi* of objects of natural history which I received from my excellent friends the missionaries of Old Calabar, was a small monkey in spirits, sent by the Rev. W. C. Thomson, who is now stationed at a place called Ikoneto, somewhat higher up the Calabar River than Creek Town. Mr Thomson furnished me with no details regarding it, but his companion and fellow-labourer, Mr Hewan, who is at present on leave in this country, on its being shown to him, at once recognised it as a little pet which was kept by Mr Thomson, and which had probably died after his (Mr Hewan's) departure. The little creature is interesting in various ways: it is interesting from its minute size, being no larger than a common mouse, and I believe is the smallest species of monkey yet known. It has a special interest in the eyes of the naturalist, from exhibiting in a characteristic manner some structural peculiarities of the group of monkeys to which it belongs; and in its domesticated state, its affectionate and playful disposition invested it with an interest of another kind in the eyes of those to whom it was attached.

With its friends it was very familiar, and used to run over their persons with perfect freedom. A favourite place of refuge was up the coat-sleeve of its master, and a still more frequent retreat was under his whisker, and between it and his shirt-collar.

It belongs to the *Galagos*, a genus peculiar to West Africa, with the exception of one species said to be found in Madagascar. The two species nearest to it are the Madagascar species *Galago Demidoffii*, and one found in Senegal, the *Galago Senegalensis*, both of Geoffroy St Hilaire. As to the former, it is at once distinguished by the size of its ears, which in that species are not a third of the size of the other. The *Galago Senegalensis* resembles it more, but is distinguished by the points of difference which I shall presently mention.

Before specifying these, however, I ought to mention that I did in this instance, as I usually do in cases of doubt, avail myself of the valuable assistance of the officers of the British Museum to acquire greater certainty on the point, and sent my specimen to my friend Dr J. E. Gray for comparison with the species of *Galago* in that establishment; and he tells me that my species is very like the *Galago Senegalensis*, and suggests that my specimen may only be a very young one of it. This suggestion, however, proves incorrect; an examination of the bones (the fontenelles are closed) shows that my specimen is adult, although not an old individual. Dr Gray, however, admits that the species which have been described are very difficult to distinguish, even with the skins before one; and the difference between stuffed specimens, in which usually the ears have shrunk in drying, and the body expanded in stuffing, and one in spirits, where the body has, on the contrary, rather collapsed, and the colour of the fur assumed a different hue, joined possibly (although he does not say so) to the feeling, that a public officer occupying his position should be cautious in giving an opinion on imperfect data, has prevented him from declaring an opinion on the subject. I am withheld by no such consideration; and being thus thrown back upon the descriptions of the authors who have established the different species, it is from a comparison of these that I have come to the conclusion that they are distinct.

The *G. Senegalensis* is figured and described by Audebert in his fine work on monkeys, and on comparing his figure and description with my specimen, I find the following points of difference. My species is only half the size of *G. Senegalensis*, the limbs are much more delicate and finely formed; and the colour is quite different,—that of the *G. Senegalensis* being a sort of orange tawny yellow, while my species is mouse-coloured: in both, the feet and under parts are pale. This statement of the colour is, however, not to be too much relied on, because my specimen being in spirits makes the colour of the fur appear somewhat different from what it is when dry. The ears of the *G. Senegalensis*, also, are said to end in a pencil of hairs, which is not the case with this species. The third finger in my species is the longest; while, if Audebert's figure of *G. Senegalensis* be correct, it is the second which is the longest. In Audebert's figure, also, the tail is represented as round and bushy; while in mine the fur is disposed rather flatly, and separated like the plume of a feather, as we see in some of the flying squirrels.

These particulars appear sufficiently to distinguish the two species; and the accompanying figure, of the size of nature, from the accurate pencil of our accomplished naturalist Dr Greville (who, I may mention, concurs in my opinion), will, I think, satisfy any one who compares Audebert's figure with this, that the two are distinct. My animal is most probably another species alluded to by Audebert, and mentioned but not described by Adanson as "seulement un peu plus grosse comme un souris." Referring to this resemblance to a mouse, I propose to distinguish it by the name of *Galago murinus*.

The generic characters are well shown in it, and one or two of these exhibit structural peculiarities which are worthy of a short notice. All that is known about the food and habits of the genus is, shortly, that it is frugivorous and insectivorous. A glance at the teeth, which will be found figured on the plate, shows at once that it is much more of the latter than the former; and the curious curved projecting nail on the first finger of each hind-foot (not counting the thumb as a finger), doubtless has reference to its mode of procuring insects. At least we may infer this from some particulars mentioned in an interesting notice of the habits of the aye-aye of Madagascar, communicated, on 7th April last, by the Honourable Dr Sandwith, through Professor Owen, to the Linnæan Society, and which will be found in the number of their Journal for July 1859. I need scarcely remind the reader that the aye-aye is, like our animal, a monkey of small size, and, like it, provided with one peculiarly-constructed finger on each hand or fore-foot. It is the second finger, and it is more than double the length of the rest, and less than half the thickness; in fact, it looks just like a long bent wire of moderate thickness. The use of this long finger was hitherto a puzzle; but Dr Sandwith having procured a living specimen, which he kept in a cage in the Mauritius, happily ascertained its use in the following manner.

Unlike the *Galago murinus*, which has the teeth small, and the canines very slightly developed, the aye-aye has very formidable canines, they being nearly as large as those of a young beaver. These it employed in gnawing the wooden bars of its cage, to such an extent as to render it necessary to protect them. Dr Sandwith observing this, thought of tying some sticks over the woodwork, that it might gnaw these instead, and put into its cage, for this purpose, a number of branches which happened to have been bored by a large and destructive grub called *moutouk*. "Just at sunset," says Dr Sandwith, "the aye-aye crept from under his blanket, yawned, stretched, and betook himself to his tree, where his movements are lively and graceful, though by no means so quick as those of a squirrel. Presently he came to one of the worm-eaten branches, which he began to examine most attentively, and bending forward his ears, and applying his nose close to the bark, he rapidly tapped the surface with the curious second digit, as a woodpecker taps a tree, though with much less noise, from time to time inserting the end of the slender finger into the worm-holes as a surgeon would a probe. At length he came to a part of the branch which evidently gave out an interesting sound, for he began to tear it with his strong teeth. He rapidly stripped off the bark, cut into the wood, and exposed the nest of a grub, which he daintily picked out of its bed with the slender tapping finger, and conveyed the luscious morsel to his mouth." As Dr Sandwith says, "The long finger is thus obviously intended to be used alternately as a pleximeter, a probe, and a scoop." Its strong canine teeth become comprehensible, and its bat-like ears in perfect harmony with the rest of its structure.

The Galago resembles the aye-aye in some of these respects, although it departs from it in others. It has the bat-like membraneous ears, and also a particular finger specially armed with an erect hooked nail (see fig. 2), but it wants the strong canine teeth with which to break into the retreat of the grub. The armed finger, however, is not the only one that is remarkable. The rest are all delicate, slender, and soft, with a broad flat pad at the tip, and also at the root, of each, and on the back furnished with a delicate flat nail shaped like that of the human species: so soft and delicate are the padded tips of the fingers, that now, after having been for some time in spirits, a gentle squeeze makes them as thin as paper; although, doubtless, in life they are plump as the tips of the fingers of the tree-frog, to which they bear in size and proportion some resemblance. A more delicate instrument for handling a fragile insect could not be imagined, and I apprehend that the true interpretation of the hooked nail and sensitive tactile tips is, that the one is for the purpose of extracting small insects from crevices, and the other for seizing them or stuffing them into its mouth.

The arrangement of the teeth also appear to bear evident marks of the adaptation of structure to its insectivorous habits. The canines and molars do not differ materially from these teeth in other insectivorous Mammifera, except, indeed, that they become wider, and more rapidly so behind; in other respects, the sharp, projecting points and irregular surface is merely a modified repetition of what we admire as so well adapted to their insectivorous diet in the mole and in the hedgehog; but it is different with the incisors: those in the lower jaw, six in number, and, comparatively speaking, pretty long, project obliquely outwards; while those in the upper jaw, only four in number, are very small, almost pointed, weak, and vertical. They are, moreover, simply inserted in the gum, the intermaxillary bones not being united at the symphysis, and there being thus no bone in which to insert them. The effect of this arrangement must be to deprive the incisor teeth of any cutting power; the inferior ones, of course, from their oblique direction, cannot cut; and the superior ones, though vertical, being mere points, and without direct opposition, are also powerless for cutting. They can therefore only be used for holding, not dividing; and this is exactly what an animal feeding on butterflies or flies would require: it does not want to bite the morsel into two, and lose a half, but, having seized its prey with its mouth, it holds it with the incisors as with a file, and with its sensitive fingers stuffs the whole into its mouth, there to be chopped into mince-meat by the broad and pointed molars.

In examining the specimen before us, I observe two glandular orifices near the eye, placed on a little tubercle situate slightly above the inner canthus, quite external to the eyelid, as represented on the plate by figure 5. Their external position would appear to be scarcely consistent with their being lachrymal orifices, although what else they can be it is not easy to conjecture; but the state of the specimen is hardly such as to admit of the glandular tubes being dissected and traced to their gland; and though they were, they would not tell us anything of its secretions or purpose.

Dr Knox in one of his publications (I think his "Races of Man") states that the yellow spot which is found on the retina of the eye in man, and is usually supposed to be peculiar to man, is also found in the monkeys of the old world, but not in those of the new world. I have carefully looked for it in the present animal, but I do not find it. My examination, however, is scarcely to be looked upon as conclusive, because, although the retina is in tolerably good preservation, it is not perfectly so; the lens has been resting on the retina, which is in part abraded, and the yellow spot, which is extremely delicate and easily obliterated, may have been upon that portion of it; but I incline to think not.

In the plate, I have given a figure of the brain in its superior, inferior, and posterior aspects (figs. 5, 7, 8); and it will be seen that the first bears a very close resemblance to the figure given by Professor Owen (Phil. Trans., 1845, "On the Brain of the Marsupialia") of the brain of another small insectivorous monkey (*Midas rufimanus*) belonging to an allied group.

The cerebral lobes have very much of the general oval outline of those of the human species, and they almost entirely conceal the cerebellum from view, but they are perfectly smooth and without convolutions.* This is the character of all the small monkeys in this group. Now, I cannot help thinking that this is a very fair opportunity of testing the worth of the chief character which Professor Owen has brought into prominence in his proposed new arrangement of the Mammifera, as when any deviation from a usual character occurs in what we regard as members of the same family, a better occasion cannot be wished for testing the value of an arrangement or definition mainly founded upon such character.

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* There are some faint traces of anfractuosities, but these are merely the impressions or channels left by the meningeal and other superficial arteries of the cranium.

Professor Owen's characters drawn from the brain, so far as they apply to the present subject, depend on whether the lobes of the cerebrum be convoluted or not, and whether or not they extend over the cerebellum; those which have them convoluted he calls *Gyrencephala*, a section which contains the great majority of the Mammifera,—indeed everything, with the exception of man, the marsupial animals, and the Insectivora, Cheiroptera, Rodentia, and Bruta; the latter—viz., the Insectivora, Cheiroptera, Rodentia, and Bruta (type, Pangolin)—compose his *Lissencephala*, or those with smooth lobes of the cerebrum. Now it is clear that, tried by this character, the Galago should be separated from the Quadrumana and ranged with the *Lissencephala*. But this Professor Owen does not propose. He considers their affinity to the other Quadrumana through the form of the cerebrum, the position of the cerebellum, the quadrumanous habits and form, &c., so great as to forbid their being separated from them; and that they must be looked upon as one of those exceptional deviations which meet us in every attempt we make to define natural objects. He does not even view them as the transition between the Quadrumana and Insectivora, which he places far apart; the former at the commencement of the *Gyrencephala*, and the latter at their termination among the *Lissencephala*. He sees their affinity with the latter, but, placing them among the Quadrumana, he must of necessity separate them from the *Lissencephala* by the whole of the *Gyrencephala;* in other words, three-fourths of the Mammifera. No doubt this is only one of the difficulties which we have to encounter whenever we attempt to make a lineal arrangement or system for Nature. Her system is not lineal, but the most complicated network, like a ball with threads passing through it, connected in every direction, and ramifying and divaricating in apparently the most inextricable confusion; therefore we can never show the affinities of nature by a chain or linear system. But admitting it to be hopeless to do so, the advantage—indeed the necessity—of having some such arrangement (artificial though it be) as mechanical help to preserve ourselves from worse confusion, is felt so sensibly by the student that we must adopt one, even while we know that it gives a false, or at all events does not give a true, view of the affinities of the objects so arranged; and if we adopt any one character of importance as our principal guide in making such artificial arrangement, we are often compelled either to abandon our chief character, or, if we stick to it absolutely, to involve ourselves in some manifestly unnatural collocation. Now, there is certainly no manifestly unnatural collocation in placing the *Galagos* and their allies with the Insectivora and Rodentia. In many respects they correspond. They (the *Galagos*) have the teeth of a hedgehog (that is, typically—specially, I have already shown their exceptional peculiarities); they have the tail of a squirrel, they have the eye of a dormouse, and the ear of a bat; and, like all these, their cerebral lobes are free from convolutions. The characters of the placentation, on which Gervais places the Insectivora in immediate sequence to the Quadrumana, is not known in the *Galago*, so far as I can learn, but may very possibly be the same as that of the Insectivora; and even although it were not, but exactly the same as that of the other Quadrumana, that very character (although not looked on in this light by Owen) is so far allied as to have led Gervais and Milne Edwards to the belief of their affinity. To join the *Galagos* to the Insectivora, therefore, does not shock me more than to remove the Insectivora from their position next the Quadrumana.

To remove the Galagos, however, from the Quadrumana would, I apprehend, be too violent a dislocation to meet the approval of most men. I agree with Professor Owen in retaining them there, notwithstanding that they do not possess the Gyrencephalic cerebrum—unless, indeed, we should erect a separate order for these Lissencephalous and insectivorous Quadrumana, which might perhaps be better; but I differ from him as to the next step, and go along with those who place the Insectivora immediately after the Quadrumana.

The result of thus testing Professor Owen's proposition by the Galago's brain is by no means to diminish our ideas of the value and importance of the Encephalic organs as a character, nor of the obligations science is under to him for furnishing us with it. It is so like presumption on my part that I hesitate to differ from him at all; but if I may venture to express my opinion, I should say that my chief point of difference from him is, that he has attempted to give the character in question a wider application and more extended force than I think it can bear. In no arrangement, as it appears to me, can any one over-riding character be permitted to control all the rest. A true definition is the union of many characters, and I would almost have preferred that he had chosen some names bearing less of the character of a definition drawn from one character than Gyrencephala, Lissencephala, and Lyencephala; we could then have equally given full weight to the cerebral characters, and we could with less reluctance have made such interpolations from the other sections as other special characters would seem to justify.

But I am extending my proper subject into one which would require much greater deliberation and deeper consideration than befits an incidental notice of a new monkey, or quasi-monkey, and must beg my readers to excuse the digression.

Explanation of Plate XI.

Fig. 1. *Galago murinus* (natural size).
2. Hind paw (natural size).
3. Upper surface of mouth seen from below (magnified twice).
4. Lower jaw.
5. Eyelids showing the two glandular orifices (magnified twice).
6. Superior view of brain.
7. Posterior do.
8. Inferior do.

*Joule's Unit verified.** By James P. Espy.

If we imagine an air-tight piston to move without friction in a cylinder 780 feet long, containing 780 cubic feet of air at the temperature of zero, of half atmospheric density, to be condensed into half the space by a force of 7½ × 144 pounds falling 390 feet—that is, through half the length of the cylinder—the air, by my experiments with the double nephelescope, will be heated by the condensation 76°.

As this is effected by a weight of 7½ × 144 pounds falling 390 feet, and as the air thus heated weighs $31\frac{26}{100}$ lbs., it may be used to test Joule's unit. $7·5 \times 144 \times 390$ is equal to the mechanical power of 421200, which heats 31·25 pounds 76°; therefore, to heat one pound 76°, will require $\frac{421200}{31·25} = 13478$, and 13478 being divided by 76°, gives 177·3, the unit for air, or the number of feet one pound would have to fall to heat one pound of air one degree; and from this unit and Joule's unit the specific caloric of air may be ascertained, supposing it to be unknown, or incorrectly ascertained by direct experiment; being divided by Joule's unit, 772, gives 17·42, the number of degrees one pound of water would be heated by the operation, and also the number of degrees one pound of air would be heated, if air had the same specific caloric as water; but as the specific caloric of bodies is inversely as their power of being heated, divide 17·42 by 76°, it will give 0·229, the specific caloric of air. Now, as my experiments with the double nephelescope give the specific caloric of air, 0·218, and Regnault's 0·23; and as Joule's unit brings out an intermediate number, Joule's unit is probably correct, and certainly cannot be altered till some of the elements from which I have confirmed it are altered by more careful experiments, or more careful calculations from the same elements. I have never seen how Joule experimented in obtaining his unit; but it will be gratifying to that gentleman to learn that his result is confirmed in so simple a manner by my experiments, as it certainly is gratifying to me to find that my law of cooling, by the expansion of air, is in perfect harmony with his most beautiful principle. I have not M. Regnault's determination of the specific caloric of air before me; but according to my recollection, it is between 0·23 and 0·24, and my experiments with the double nephelescope make it 0·218. If we assume Joule's unit as accurate, using that as an element, the specific caloric of air is exactly 0·229.

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* Communicated by R. Russell, F.R.S.E.

It follows, also, from Joule's principle, that we may easily find the temperature of air condensed into double, treble, &c. densities, if we take Regnault's authority for granted, that the specific caloric of air is the same at all densities and temperatures.

For if we suppose a cylinder of air at half density, with a temperature of zero, to be condensed by pressure into half the space, it will be heated by the process 76°; and if it is condensed into one-fourth the space, it will require twice the force moving through one and a-half times the space, and will thus produce an increase of temperature of 3 × 76°; and if it is condensed into one-eighth the space, it will require four times the force moving through one and three-fourths,—that is, 7 × 76°; the next duplication will increase the temperature to 15 × 76°; the next to 31 × 76°, &c.; so that at the distance of thirty-five miles below the surface of the earth, the air, by its own pressure, supposing it doubled its density every three and a-half miles, would be 1024 times as dense as at the surface of the earth, and 153,748° hot.

This calculation is made on the supposition that M. Regnault's experiments have proved that the specific caloric of air is the same for all densities and temperatures. According to my experiments with the double nephelescope, the specific caloric of air diminishes as the density increases, a double density giving six and a-half per cent. less specific caloric; the heat, therefore, if my experiments are correct, is much greater than the estimate above. Certain it is, when double density was used, the cold of expansion into double space was 81°, whilst in common air the cold was only 76°.

The specific caloric of steam may also be calculated from the mechanical unit of heat. For, as a column of air 780 feet long, of one square foot area, contains 62½ pounds in weight, the quantity of steam in pounds which is contained in an equal column can be calculated by taking $\frac{5}{8}$ths of its weight, and diminishing that quantity in proportion to its temperature above zero. For example, at one-half density, the column of air at zero of the above size weighs 31·25 pounds, $\frac{5}{8}$ths of which = 19·54 pounds, and this diminished by $\frac{180}{628}$ths of the whole, is 13·95 pounds, which is the weight of a column of steam at half atmospheric pressure and 180° in temperature. Now, if the area of the end of the cylinder, 144 inches, is multiplied by 7¼ pounds, which must fall 390 feet to produce double density in the steam, and thus raise its temperature 32°, it will make 10,880; and this multiplied by 390, makes 424,320, and is the power which heats 13·94 pounds of steam 32°; consequently, $\frac{424,320}{13·94}$ will be required to heat one pound 32°; and if this last number, $\frac{424,320}{13·94}$ be divided by Joule's unit, 772, it will give the number of degrees a pound of water would be heated by the process, 39°·255; and as the specific caloric of bodies is inversely as their powers of being heated, if 39°·255 be divided by 32°, it will give 1·223, the specific caloric of steam at atmospheric pressure and 212° temperature.

In like manner I have calculated the specific caloric of steam for the several densities and temperatures below.

The first column contains the pressures in atmospheres; the second, the temperatures; the third, the number of pounds of steam in the column to be heated by the condensation; the fourth, the mechanical power required to produce the condensation; the fifth and last, the specific caloric of steam at the different densities.

Atmospheres.	Temperatures.	Pounds of Steam.	Mechanical Power.	Specific Caloric.
½ to 1	212°	13·95	421,200	1·223
1 to 2	248·5	26·52	842,400	1·067
2 to 4	293·4	50·23	1,684,800	0·965
4 to 8	343·6	94·40	3,369,600	0·916
Inches.				
0·2 to 0·4	52·	0·2427	11,232	1·50

From this table, unless there is some miscalculation, it appears that the specific caloric of steam diminishes as that of air with the increase of density. There is, in fact, a diminution of twenty-five per cent. from one atmosphere to eight in density, or rather in tension, for with eight times the tension, the density is only about 6·7 times greater.

It appears, also, from the principle in question, that in condensing air, the higher the initial temperature the greater will be the heat produced by condensation, so that if air should be used at 448° above zero, a condensation into half the space would heat it 2 × 76°, and so in proportion.

In making these calculations, I should perhaps have used Regnault's coefficient, 461°, rather than the old one, 448°; but as my object is rather to show how Joule's unit, and the specific caloric of air, and the unit for air, 177·3, are connected, so that any one of these quantities may be used to correct the other, than to settle any of them definitely; I have contented myself with the loose manner in which these calculations have been made.

On some of the Constituents of the Tar obtained by Destructive Distillation of Brown Coal. By Mr MAX DÜRRE of Magdeburg.

The tertiary formations of the North German plain abound in this kind of coal, which, although it bears decidedly the features of coal, as known in this country, differs in its outward appearance to such an extent as to justify the special name. We are, however, not entitled to oppose this coal as a class to the common coal as another class, but must look upon it as forming the last links of that great chain of relics of former vegetations which begins with the anthracites of America, and will be extended by the layers of peat and turf of the present day, According to their age, we find these tertiary coals distinguished from each other by their ultimate composition,—by colour and other physical properties, among which the differences in structure are pre-eminent. Many of them preserve the fibrous arrangement of their particles so well that botanists are able to recognise the genus of those antediluvian plants without difficulty; whereas in other cases the destruction of the ligneous fibre is so advanced as to baffle all efforts of the most skilful observers to trace their origin. This latter class is termed brown coal, *par excellence*, the first one being improperly called "bituminous wood." Brown coal occurs either in large lumps of considerable coherence, and of a black or brown colour, or in an earthy damp state, varying in colour from black to reddish-yellow.

The coal which furnished material for the present examination occurs at the northern slope of the Thuringian mountains, where it is worked on a considerable scale. Coherent pieces are not common ; it appears usually in a coarse, powdery state, of a light-brown colour, which frequently passes into the reddish tint above mentioned.

The coal is submitted to destructive distillation with the view of obtaining liquid and solid bodies for illuminating purposes, and I may be permitted to sketch in a few lines the method which is found best to effect this end.

The coal is heated in cast-iron retorts to a dull redness, the gases and vapours being allowed to descend through iron pipes, from each retort, into a common pipe of considerable dimensions. The tar is discharged into a cistern, the gases being permitted to circulate through vertical pipes, in order to condense the tar effectually. The tar then undergoes distillation in cast-iron stills, and the distillate is collected in three separate receivers, the first one being removed as soon as the distilling fluid is of 0.833 sp. gr. The second stage is carried on until a drop solidifies on cooling. The residue in the still is like asphalte, very similar to what is obtained in distilling crude coal naphtha. Only a tenth of the tar is got in the first part of the distillation, the greatest portion coming over in the third stage. The first fraction is treated alternately with monohydrated sulphuric acid and caustic soda of 70° Twd., well washed and re-distilled. The second fraction undergoes the same process of purification, but with a larger quantity of sulphuric acid and soda. Another distillation makes it ready, and it is sold under the name of heavy photogen, its specific gravity being considerably higher than that of the light photogen,—viz., 0·876 and 0·827 at 15° C. The portion coming over last contains the paraffine, which can be obtained by crystallizing, and purified by several mechanical operations.

The ordinary or light photogen occurs in commerce in the form of a yellow limpid fluid, the odour of which is quite different from any of the known products of dry distillation, and is at once empyreumatic and ethereal, but by no means disagreeable. Its specific gravity is 0·8271 at 15° C. It burns with a white smoky flame, and is soluble in alcohol and ether. Ebullition commences at 135° C., but no constant boiling-point could be obtained. When the thermometer indicated 215° C. nearly all the fluid had distilled over, and the small residue in the retort then underwent decomposition; white fumes were given off, and the distillate obtained a decidedly disagreeable smell, whilst the fluid blackened very much.

There being very little doubt that the naphtha was a mixture of several bodies, it was considered necessary to make a tolerably complete fractional distillation before any attempt could be made to ascertain their nature. Wurtz's apparatus was employed with great advantage. The receiver was changed every 5° C., and as soon as decomposition became visible, distillation was stopped and the black residue altogether removed. The lower fractions were at first perfectly colourless, but turned very soon light-yellow; the fractions of a higher boiling-point came over with a yellow tint, which changed after the lapse of some hours into brown. This, however, lessened rapidly as the number of rectifications increased. At length, after twelve rectifications, colourless fractions, at almost the constant boiling-point, were obtained. The depression of the point of ebullition was very considerable in the first rectification, the first fraction beginning to boil at 112° C. As the quantity of the residues in the flask were always very small, it can scarcely be doubted that their presence was owing to an imperfect purification of the naphtha—a supposition which afterwards proved to be correct. The lowest fraction began to boil at 75° C., but between this point and 100° C. the fractions were too small to admit of their true nature being ascertained. Preliminary experiments made with the original substance, as well as with the fractions, boiling between 80° and 90° C.

proved the absence of benzole—results which might have been anticipated, considering the previous purification in the manufactory.

Several closely approximating analyses of some of the fractions showed a loss oscillating between 0·5 and 1·0 per cent., which was doubtless owing to the presence of an impurity containing oxygen. A plan therefore required to be adopted to remove this, without acting upon the hydrocarbons. I resorted to the same means which had already been employed in the manufactory,—viz., concentrated sulphuric acid and caustic soda. The latter substance did not prove effectual, there always being a loss of 0·5 per cent. in the analyses, even after long-continued treatment. Sulphuric acid, however, gave full satisfaction, as the impurity could be thoroughly removed without visibly diminishing the quantity of the naphtha. The crude hydrocarbons were shaken with about a tenth of their volume of sulphuric acid for some minutes, the acid then removed by means of a pipette, and this process repeated as long as any coloration of the acid could be observed. A careful washing served to remove the oil of vitriol and the new-formed acids. If the slightest trace of acid was left, the fluid invariably became black in the following distillation; otherwise a perfectly colourless fluid came over, which did not colour in the least, even after the lapse of many weeks. Sodium heated in it kept its brilliancy after the coating of soda was removed. This mode of purification was applied to all the fractions with perfect success; the higher ones, however, required a renewed treatment with acids, which was almost unnecessary with regard to the lower ones. The naphtha had now entirely lost the empyreumatic smell, and acquired a very pleasant odour, which even improved with the rising boiling point.

Although fluids were obtained, the boiling points of which were almost perfectly constant, they consisted, nevertheless, of more than one substance, which was proved by their deportment with fuming sulphuric acid. When two ounces of the naphtha, with an equal volume of fuming sulphuric acid, were agitated in a stoppered bottle of eight ounces capacity, the fluid blackened intensely, and sulphurous acid may easily be observed by the smell; the temperature rises but little, even when the mixture is violently shaken. After a short time the fluid separates again into two layers; the naphtha now being diminished by more than half, the process is repeated with fresh quantities of sulphuric acid till no farther coloration can be observed. The acid being removed for the last time, the fluid is washed, until the water, siphoned off, does not affect blue litmus paper. The well-dried hydrocarbon, after being distilled over sodium, shows an entirely different odour from that noticed in the mixture; it is very volatile, even at low temperatures, and burns with a white, but by no means smoky flame.

The indifference of the hydrocarbon towards fuming sulphuric acid made it almost certain that it belonged to the alcoholic series of the general formula, $C_n H_{n+2} = 4$ volumes of vapour, which became evident by its composition and its deportment with fuming nitric acid, as it remained unattacked, and the nitric acid, after diluting with water, did not show the slightest trace of a dissolved nitro-compound. These experiments were made upon a fraction, boiling between 105–110° C., and the indifferent hydrocarbon came over almost entirely at 108° C. This being exactly the boiling point of Kolbe's butyle, obtained by electrolysis of valerianic acid, the body in question might have been expected to be identical with butyle. Williams,* however, showed that the indifferent hydro-carbon obtained from a fraction of Boghead coal naphtha, boiling between 107° and 110° C., was not pure butyle, this boiling at 119° C. The near relation of the sources made it highly probable that the butyle from brown coal would possess the same boiling-point as that from Boghead coal, and this was placed beyond doubt by the numbers found for the respective vapour densities of the body, boiling at 108° C. (I.); and of that obtained from a fraction boiling between 118°–120° C. (II. and III.), the point of ebullition last mentioned remaining steadily at 119°5 C.

I. Excess in the weight of the balloon, 0·305 gr.
Temperature of air, 14° C.
" " the bath during sealing, 149° C.

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* Proceedings of the Royal Society, January 22, 1857.—*Chem. Gaz.*, 1857, p. 95.

Capacity of the balloon, 177 cubic cents.
Residual air, 2 cubic cents.
Pressure, 742 m. m.
Density of the vapour, 3·59.

II. Excess in the weight of the balloon, 0·374 gr.
Temperature of air, 15° C.
" " the bath during sealing, 162° C.
Capacity of the balloon, 192 cubic cents.
Residual air, 1 cubic cent.
Pressure, 742 m. m.
Density of the vapour, 3·99.

III. Excess in weight of the balloon, 0·295 gr.
Temperature of air, 14° C.
" " the bath, 170° C.
Capacity of the balloon, 164 cubic cents.
Residual air, 2 c. c.
Pressure, 734 m. m.
Density, 3·94.

Theory requires—

16 C = 16 volumes, = 13·26752
18 H = 36 volumes, = 2·48760

Density of 4 vols. of vapour, = 15·75512
" 1 vol. " = 3·939

I. Boiling at 108° C.	Boiling at 119°5 C.		Theory.
I.	II.	III.	$C_{10}H_{18}$ = 4 vol.
3·59	3·99	3·97	3·939

The following numbers were obtained in analysis of the hydrocarbon, boiling at 119°5 C.:—

1.—0·215 gr. substance gave—
0·663 gr. carbonic acid, and
0·308 gr. water.

2.—0·193 substance gave—
0·596 carbonic acid, and
0·276 water.

	I.	II.	Theory.		
C =	84·04	84·22	96	16	84·21
H =	15·91	15·89	18	18	15·79
	99·95	100·11	114		100·00

After the presence of butyle was thus proved, that of amyle might be anticipated. I proceeded therefore at once to examine the fractions boiling between 155 and 165. The indifferent hydrocarbon obtained by the same processes as applied to the lower fractions was submitted to a few fractional rectifications, which were considered necessary on account of the mercury rising by 12° C. in the first rectification. After three rectifications, the fluid boiled steadily at 159°5 C., which number almost exactly agrees with the boiling-point of the amyle found by Williams, viz., 159° C., and only one degree and a half higher than that found by Wurtz.

Analyses gave the following numbers :—

1.—0·214 gr. substance gave—
0·6625 carbonic acid, and
0·3025 water.

2.—0·199 gr. substance gave—
0·6155 carbonic acid, and
0·281 water.

	I.	II.	Theory.		
C =	84·43	84·35	120	50	84·51
H =	15·71	15·69	22	22	15·49
	100·14	100·04	142		100·00

A determination of the vapour density proved that the body boiling at 159° C. had not only the per-centage composition, but the same condensation as the radical amyle, thus :—

Excess of weight of the balloon, 0·554.
Temperature of air, 14° C.
" " the bath, 186° C.
Capacity of the balloon, 230 cubic cents.
Residual air, 1 c. c.
Pressure, 742 m. m.
Density, 4·83.

Theory requires—

20 C = 20 volumes = 16·5844
22 H = 44 volumes = 3·0404

Density of 4 volumes of vapour,	19·6248
Density of 1 volume of vapour,	4·906

Boiling at 159°5 C.	Theory = to 4 vol.
4·83.	4·906.

It has been already mentioned that the fractions which probably contain propyle were too small to allow any detailed examination; it may suffice to state, that there are hydrocarbons unacted on by sulphuric acid, and apparently in comparatively greater proportion than in the fractions of a higher boiling-point. I hope to be soon able to communicate satisfactory results with regard to this body. Caproyle, likewise, could not be obtained; the last fractions I had at command boiling at 180–185° C., whereas caproyle boils at 202° C.

I waive altogether the question, whether the hydrocarbons obtained by destructive distillation, and possessing the composition of the alcohol radicals, are identical with the bodies obtained by electrolysis of the fatty acids, and by the action of sodium upon iodide of butyle. It is to be regretted that the hydrurets of the radicals are so little known at the present. Possibly more correct views would be derived from their better knowledge.

The study of the sulpho-compounds, formed by the action of fuming sulphuric acid upon the mixed hydrocarbons, did not promise satisfactory results; it is true a sulpho-acid was formed, the salts of which, with baryta, crystallized pretty readily; but there were always evidently more than one kind of crystals mixed, the separation of which would have required larger quantities of material than I had at my disposal. I therefore resorted to nitric acid, with the view of preparing nitro-compounds which might be obtained in a pure state, either by distillation or by crystallization. Two drachms of strong fuming nitric acid were poured into a stoppered bottle of four ounces capacity, and part of the fraction, boiling between 100° and 105° C., added in small quantities, the bottle being immersed in cold water, so as to ensure the lowest possible temperature. When the hydrocarbon came into contact with the acid, a hissing sound was produced, with slight evolution of red fumes; as soon as the mixture became a little warm, the fumes were much increased, and the action upon the naphtha takes place with an almost explosive violence. It is indispensably necessary to keep the nitro-compound in solution by the acid, or the indifferent hydrocarbon will be dissolved in the nitro-compound, which forms a homogeneous layer above the acids; when poured into water, the compound nevertheless does not float on the surface, the quantity of the hydrocarbon being too small to balance the greater density of the nitro-compounds. But when this is dissolved in the acid, a separation takes place very easily, the hydrocarbon rising to the surface, where it forms a brilliant green layer. The nitro-compound is then thrown into water and well washed; it shows in this state a red-brown colour; the smell recalling nitrobenzole, although less agreeable. Analysis did not give results which allowed a conclusion to be drawn regarding the original hydrocarbon; there evidently being a mixture of mono and binitro compounds. The body began ebullition at 220° C.; the greater part of it went over between this and 230° C.; and then the mercury rose very quickly till near 300° C., when thick vapours appeared, whilst the residue in the retort blackened; thus the last portion was mixed with products of decomposition, and did not admit of any inferences to be drawn from its examination. The first distillate was subjected to some rectifications, and a fluid obtained, which had an almost constant boiling-point at 225° C. This agrees with the boiling-point of nitro-toluole, as found by Deville. Analysis leads likewise to the formula $C_{14}\ H_7\ NO_4$, as may be seen from the following numbers:—

1.—0·364 gr. substances gave—
0·7755 gr. carbonic acid, and
0·170 gr. water.

2.—0·391 gr. substance gave—
0·876 gr. carbonic acid, and
0·192 gr. water.

	I.	II.	Theory.		
C	61·13	61·17	84	14	61·31
H	5·46	5·45	7	7	5·11
N	...	...	14	1	10·22
O	...	...	32	4	23·36
					100·00

The fluid, after distillation, was of a golden colour, but darkened soon into an orange-red.

It is obvious, that dinitro-toluole was formed at the same time, although precautions were taken to prevent the mixture of acid and naphtha from becoming hot; and the distillate which came over at 300° chiefly consisted of this body. It may now be fairly inferred, that the hydrocarbon from which the nitro-compound in question originates is toluole, an opinion which is strengthened by the circumstance, that the fraction worked upon boiled between 100° and 105° C.; and Church* found 103·7 C. to be the correct boiling-point of toluole.

As the other homologues of benzole could now be anticipated almost with certainty, I proceeded, therefore, at once to seek for xylole, and selected a fraction boiling between 125° C. and 130° as the starting-point. Fuming nitric acid, applied in the way above detailed, did not prove so efficacious as in the preceding case. It is true a liquid compound was observed, but no formula could be derived from the numerous analyses which were made. During distillation, decomposition took place; the contents of the retort became black, and the distillate was of a smell quite different from that usually observed in bodies of this class. These results rendered it evident that the only means of obtaining definite products would be to extend the action of the nitric acid: at first, the solution of the naphtha, in fuming nitric acid, was boiled, and then precipitated by water; but no precise difference was observable, the composition still showing the product to be a mixture of mono and binitro compounds.

At length, a mixture of fuming nitric acid and sulphuric acid gave satisfactory results. If both acids, in equal volume, be poured into a flask, and the naphtha allowed to drop out of a pipette with a narrow aperture, the fluid becomes hot, and each drop occasions a sharp hissing sound. After some minutes, small crystals become visible on the glass; as soon as they appear, no more naphtha is to be added, but the flask allowed to cool down gradually. The walls are then covered with yellow or reddish-brown crystals, which sometimes form a pasty mass, when rather much naphtha is used. This can be entirely avoided if the mono-hydrated acid is replaced by fuming acid, in which case it requires great excess of naphtha to produce that effect. I always found the stronger acid more useful, as the subsequent purification of the crystals is attended with difficulties when they are mixed up with a fluid compound. The solid body was then removed and washed; adherent particles of the liquid compound were separated, by pressing between blotting-paper and washing with hot alcohol, the solid being but sparingly soluble in that menstruum. Strong nitric acid and oil of vitriol dissolve it readily when heated; and on cooling, brilliant crystals, sometimes of considerable length, are obtained. They are of a yellowish colour, with a beautiful green lustre; insoluble in water, sparingly soluble in cold alcohol, more in hot alcohol and ether. From the solution in strong acids the body may be precipitated by water, in the shape of a white powder with a yellowish tint. It melts at a low temperature, and can be sublimed without decomposition.

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* Phil. Mag., [4] ix. 256.

Analysis leads to this formula: $C_{16}H_7N_3O_{12}$, viz., trinitroxylole, as may be seen from the annexed numbers.

1.—0·240 gr. substance gave—
0·349 gr. carbonic acid, and
0·064 gr. water.

2.—0·261 gr. substance gave—
0·377 gr. carbonic acid, and
0·073 gr. water.

3.—0·291 gr. substance gave—
46 cubic cent. of nitrogen at
756 m. m. pressure, and
20° C. temperature.

4.—0·498 gr. substance gave—
78 cubic centim. of nitrogen at
742·5 m. m. and
18° temperature.

	I.	II.	III.	IV.	Means.
C	39·66	39·39	...	...	39·53
H	2·96	3·12	...	...	3·04
N	...	...	17·95	17·79	17·87
O	...	...	...	...	...

	Theory requires		
C	96	16	39·84
H	7	7	2·90
N	42	3	17·40
O	96	11	39·86
	241		100·00

It may be mentioned that this substance has not hitherto been analysed.

Cumole was to be looked for in the fractions boiling between 145–150° C. In doing so, I subjected the naphtha to the process detailed with reference to xylole. The product was a body very similar to trinitro-xylole in its physical properties, and I could fairly expect it to be trinitro-cumole. Analysis, however, gave numbers exactly agreeing with this formula: $C_{18}\,H_9\,N_3\,O_{14} = C_{18}\,H_9\,(NO_4)_3\,O_2$, namely,—

1.—0·274 gr. substance gave—
0·402 gr. carbonic acid, and
0·088 gr. water.

2.—0·252 gr. substance gave—
0·368 gr. carbonic acid, and
0·082 gr. water.

3.—0·301 gr. substance gave—
0·440 gr. carbonic acid, and
0·099 gr. water.

4.—0·556 substance gave—
74·5 c. c. of nitrogen at
10° C., and 759 m. m. pressure.

	I.	II.	III.	IV.	Means.
C	40·01	39·83	39·99	...	39·94
H	3·56	3·62	3·65	...	3·61
N	...	...	...	16·01	16·01
O	...	...	...	...	...

Theory requires

C	108	18	39·85
H	9	9	3·32
N	42	3	15·50
O	112	14	41·33
	271		100·00

This formula belongs to a homologue of picric acid; but the properties of the body have not been so far examined as to decide whether it is really a homologue or isomeric with it. It may be added, that the alcoholic solution did not change litmus paper. It is insoluble in water, little soluble in alcohol and ether; readily in strong acids, from which it is precipitated by water. Although homologues of picric acid are not yet known, and picric acid itself is not obtained from benzole by mere oxidizing agents, there can scarcely be a doubt about the composition of the body in question ; I regret, however, that want of material prevented me from throwing more light upon this interesting compound.

Cymole is evidently present, and in comparatively large proportion, which becomes obvious by the large quantity of the fractions boiling between 165° and 175° C. But the analytical results obtained are not sufficiently in accordance with each other to establish formulæ for the nitro-compounds produced.

The body which has been mentioned as containing oxygen is only an impurity of the naphtha. I got some of it as a residue in distillation of the last fractions; but as rapid decomposition took place, when I attempted to rectify it, and the small quantity did not allow me to study its derivatives or compounds, not even an indication of its true nature could be obtained.

Thus we have essentially two series of bodies in the brown coal naphtha, namely:—

1. Hydrocarbons of the general composition $C_n H_{n+2}$, as butyle and amyle ; and,
2. Hydrocarbons of the formula $C_n H_{n-6}$, as toluole, xylole, cumole, and cymole.

The first one forms a constituent part of the Boghead coal-naphtha, of the Rangoon tar, and of the subject of the present investigation ; it therefore appears almost as general as the second series, and on this ground we may suspect its presence in the tar of common coal.

I hope to be able to give some farther communications, which are intended to comprise the constituents of the heavy photogen and the rest of the non-basic bodies in the brown coal-tar.

These experiments were made in the laboratory of Dr Anderson, whom I have to thank for his kind advice during the progress of this investigation.

PROCEEDINGS OF SOCIETIES.

Royal Society of Edinburgh.

Monday, 18*th April* 1859.—Professor CHRISTISON, V.P., in the Chair.

The following Communications were read:—

1. Researches on Radiant Heat.—Part II. By Balfour Stewart, Esq. Communicated by the Secretary.

The first part of this paper describes the following groups of experiments:—

I. On the effect which roughening the surface of a body produces upon its radiation.

II. On the nature of that heat which is radiated by rock-salt at 212° F.

III. On the radiation of glass and mica at high temperatures.

The second or theoretical portion of the paper has reference to the law which connects the radiation of a particle with its temperature, and to Dulong and Petit's experiments on this subject. The instruments used, and method of using them, were almost the same as described in the first series of these researches.

With regard to the first group of experiments, it was ascertained that roughening surfaces of glass or rock-salt with emery paper until they are dim for light, neither alters the quantity nor the quality of the heat which they radiate; the surface, although dim for light, being yet specular for heat.

With regard to the second group of experiments, it was shown that heat from rock-salt at 212° F. penetrates a screen of glass or mica less easily than lamp-black heat at 212° F.

It also penetrates a screen of mica split by heat less easily than the latter description of heat; but the difference of behaviour of the two kinds of heat with regard to this substance is not so marked as in the case of ordinary mica.

So far as tested by mica, and mica split by heat, it was shown that,—

Lamp-black heat of 700° F. bears to
Lamp-black „ 212° F., the same relation as
Lamp-black „ 212° F. bears to
Rock-salt „ 212° F.

that is to say, rock-salt heat possesses greater average wave-length than lamp-black heat.

With regard to the third group of experiments, it was shown that glass or mica, by being heated, does not change in any measure its capacity for transmitting a given description of heat; for instance, cold glass transmits heat of 700° F. just as well as glass heated to 700° F. Proceeding, then, to the theoretical part of the paper, it was shown,—

1*st*, That the absorptive power of a thin plate of any substance equals its radiative power.

2*d*, That (by the third group of experiments) the absorptive power of cold glass for heat of 700° F. is the same as that of glass heated to 700° F.

3*d*, That cold glass has a greater transmissive, or less absorptive power for heat of 700° F., than for heat of 212° F.,—and hence we must conclude that "the radiation of a thin plate of glass, or other substance, at 700°, bears a less proportion to the total radiation of 700° F., than its radiation at 212° does to the total radiation of 212° F. It was also shown that this difference is more marked for a thin plate than for a thick one; and it was argued, that Dulong and Petit's law does not express the law of radiation of a material particle, but that this law, whatever it be, increases (for all bodies) less rapidly with the temperature than Dulong and Petit's law.

2. Some Observations on the Coagulation of the Blood. By John Davy, M.D., F.R.S. Lond. and Edin.

Dr Richardson, in a recent and elaborate work on the blood, an extension of a Prize Essay on the cause of the coagulation of this fluid, has endeavoured to prove that this phenomenon is of a chemical kind, depending on the escape of the volatile alkali.

The author of the paper of the above title describes three sets of experiments which he has instituted for the purpose of testing Dr Richardson's hypothesis. In all his trials on blood, he has used that of the common fowl, its properties being best adapted to the objects in view. The results obtained were briefly the following:—

1. Ammonia added to the blood in small quantities did not prevent its coagulation; in larger quantities it retarded coagulation, and rendered the blood viscid.

2. On exposing mixtures of blood and ammonia, and of water and ammonia, to the open air, the loss of weight sustained in two or three minutes—the time required for the coagulation of the blood—was hardly appreciable, using a very delicate balance.

3. The moist fibrin of the blood subjected to the action of ammonia was found to be rendered transparent and viscid, but to be very slightly soluble.

These results, and others, such as the coagulation of the blood in close vessels, and the volatile alkali not having hitherto been detected in healthy blood, have led the author to the conclusion that the phenomenon under consideration still remains an unsolved problem; and that on the ground of mere probabilities it is not easy to say which of the two chief hypotheses advanced concerning it—the chemical and the vital—is deserving of preference.

3. On the Recent Vindication of the Priority of Cavendish as the Discoverer of the Composition of Water. By Professor George Wilson.

The object of this communication was to direct attention to the recent recovery of two documents establishing the priority of Cavendish as the discoverer of the composition of water. Their importance was indicated by the late Mr Robert Brown, the botanist; and his literary executor, Mr J. J. Bennett of the British Museum, has brought them before the Royal Society of London. They are both, contrary to general expectation, published statements contained in well-known works. The first is a section of De Luc's "Idées sur la Météorologie," entitled, "Anecdotes relatives à la découverte de l'Eau sous la forme d'Air," in which the following decisive declaration occurs:—

"Vers la fin de l' année 1782, J'allai à *Birmingham* ou le Dr Priestley s'étoit établi depuis quelques années. Il me communiqua alors, que M. Cavendish, d'apres une remarque de M. Warltire; qui avoit toujours trouvé de l'*eau* dans les vases où il avoit brûlé un mélange *d'air inflammable* et d'air atmospherique; s'étoit appliqué à découvrir la source de cette eau, et qu'il avoit trouvé, 'qu'un mélange *d'air inflammable* et *d'air dephlogistiqué* en proportion convenable, étant allumé par l'étincelle électrique, se convertissoit tout entier en *eau*.' Je fus frappé, en plus haut degré, de cette découverte." (*Idées*, &c., tome ii., 1787, pp. 206–7.)

The important testimony thus borne to Cavendish's experiment having had as its object the discovery of *the source* of the water which appeared when hydrogen and oxygen are burned together; as its phenomenal result, that in certain proportions *a given weight of the gases* in question could be burned into *the same weight of water;* and as its logical induction, that *the gases had been converted into the water*, constituted Cavendish *a* discoverer of the composition of water. And as this conclusion was drawn in 1782, whilst Watt, the earliest counter-claimant of the discovery, did not draw his similar conclusion till 1783, the priority unquestionably belonged to Cavendish, who was thus *the* discoverer of the compositeness of water.

Reference was then made to the effort of Mr Muirhead to undervalue De Luc's testimony, on the plea that in another part of the

"Idées" its author declared himself to have been ignorant of Cavendish's conclusions till 1783, and not to have learned them till after he was familiar with those of Watt. It was contended, on the other hand, that De Luc's two statements were not contradictory, but perfectly reconcileable with each other,—the one referring to Cavendish's interpretation of his experiments on firing hydrogen and oxygen, which De Luc learned from Priestley in 1782; the other to Cavendish's full theory of the formation of water, which was not made public till January 1784, and which it could be shown by the Watt Correspondence De Luc did not become acquainted with till March of that year.

Special attention was drawn to the fact, that the section of De Luc's "Idées" from which the quotation was taken went over the same ground as the Watt Correspondence. This section contained the matured and authoritative publication of those views on the relative merits of Watt and Cavendish which are referred to by De Luc in the hasty private letters printed in the "Correspondence" in question, written when he was imperfectly informed on the points he was discussing, and not intended for publication. The author dwelt upon the omission of Mr Muirhead, when editing the Watt Correspondence, to point out this important fact to his readers, and the misleading effect of this omission in representing De Luc as much more the advocate of Watt's claims than in reality he was. In many respects the "Idées" supplemented the Watt Correspondence so far as the views of De Luc were concerned, and the latter work could not be understood unless read in the light of the former.

The second of the recovered documents was an extract from a Report to the French Academy, on M. Seguin's experiments on the Combustion of Hydrogen and Oxygen, dated 28th August 1790, written by La Place, in name of a Commission consisting, besides the reporter, of Lavoisier, Brisson, and Meusnier, all of whom sign it. The passage of most importance, as showing that Lavoisier abandoned in favour of Cavendish the claim he at one time preferred to be the discoverer of the composition of water, is as follows:—

"M. Macquer a observé dans son Dictionnaire de Chimie que la combustion des gaz hydrogène et oxygène produit une quantité d'eau sensible; mais il n'a pas connu toute l'importance de cette observation, qu'il se contenta de présenter, sans en tirer aucune conséquence. M. Cavendish paroît avoir remarqué le premier que l'eau produite dans cette combustion est le résultat de la combinaison des deux gaz, et qu'elle est d'un poids égal au leur. Plusieurs expériences faites en grand et d'une manière très-précise par MM. Lavoisier, La Place, Monge, Meusnier, et par M. Lefevre de Gineau, ont confirmé cette découverte importante, sur laquelle il ne doit maintenant rester aucun doute." (*Annales de Chimie*, tome viii., pp. 258–9.)

In conclusion, the author dwelt upon the brightened moral aspect of the water controversy. From De Luc's "Idées" all trace of charge against the fair-dealing of Cavendish has vanished. Lavoisier is found making full, if somewhat tardy, amends for any wrong he did the English philosopher; and as De Luc and Lavoisier testify that Cavendish had reached his famous discovery in 1782, the most uncharitable must cease suspecting that he borrowed or stole it from Watt, who had it not to offer any one till 1783.

4. On the Preservation of Footprints on the Sea-Shore. By Alexander Bryson, Esq.

The author remarked, that the impressions of the feet of birds and molluscs on wet sand were liable to be effaced by the return of the tide; and that their preservation was owing to dry sand blown into the depressions from the shore, and again covered by a layer of moist sand or mud by the return of the tide. In regard to tracks left by gasteropodous molluscs, he stated that great caution was necessary to distinguish them from those left by Nereids; and instanced the case of a foot-track of a common whelk resembling the marks made by the Crossopodia on the Silurian slates. When the track of the whelk is filled up by the dry sand blown into the depression in the *line* of progress, no difficulty is felt in recognising it as the track of a gasteropod; but should the wind blow at right angles to the track of the mollusc, a series of setæ-like markings will be observed to leeward, caused by the dry sand adhering to the moist. In this instance, a geologist would naturally assign the markings to the impression of *Graptolites priodon*, or *sagittatus;* and if the wind suddenly shifted to the opposite direction, another series of setæ would be found on the other side of the mollusc's track, and the observer would at once pronounce the marks due to a gigantic Crossopodia, or fringe-footed Annelide.

The author also stated, that the so-called rain-marks found on sandstone and Silurian slates were formed by Crustacea, and that the cusps which geologists had supposed were the evidence of the force and direction of the wind during the shower, were produced by the wind blowing dry sand from the shore, and causing a raised barrier to leeward of the depression, where there was more moisture, and consequently more adhesion of the sand.

Botanical Society of Edinburgh.

Thursday, 12th May—Professor Balfour, V.P., in the Chair.

The following donations to the Museum were announced:—From Mr M'Nab—Portraits of Allan Cunningham and Daniel Ellis. These were directed to be framed and hung up with the other botanical portraits belonging to the Society. From Dr Harvey—Portion of a tree, in the hollow trunk of which a sermon was preached to thirty persons; also, roots of a tree which were produced at a considerable height from the ground inside its hollow stem.

The Earthquake at Quito.

The following letter to Dr Balfour from Mr Isaac Anderson was read:—

"*Maryfield, Edinburgh, May* 12, 1859.

"My dear Sir,—As you expressed a desire to know the real facts as to the destruction of the city of Quito, reported in the newspapers to have been almost entire, and how it affected our excellent friend, Professor Jameson, of the university there, I have great pleasure in sending the doctor's letter to me, dated the 23d March, the day after the earthquake, from which you will be glad to hear that not only is he unscathed in person, but also little injured in his property. And as he mentions that comparatively *few lives were lost*, the newspaper account of 5000 of the inhabitants having been killed must be a myth. From Dr Jameson, however, saying that this earthquake has been the most severe he ever experienced, it must indeed have been an appalling one; for he has resided now thirty years continuously in that volcanic region, where these things are of frequent occurrence.

"You will observe that I am still receiving seeds of alpines, &c., from that interesting locality, some of which I will have great pleasure in sending for the inspection of the members of the Botanical Society, as they come into flower. With this I send a berberis, set with flowers, but not yet opened. I also send a vaccinium, with some of the spikes thickly set with pretty roseate blooms. It grows at 10,000 to 11,000 feet of elevation on the Andes, and bears eatable fruit.

"What, perhaps, will be most interesting to your Society will be such alpines as the Gentian, Swertia, Silene, Castilleja, Potentilla, Œnothera, Gaultheria, &c,, of which I have had various species sent me, now coming forward.—I am, very faithfully yours,

"ISAAC ANDERSON."

The following is an extract from Professor Jameson's letter above referred to, dated 23d March 1859:—"Since yesterday morning we have been dreadfully alarmed by a violent shock of an earthquake, which has overthrown and ruined many of the public buildings, and damaged more or less *all* the houses of private individuals. The churches have suffered most, but fortunately *few* lives were lost. The earthquake (the most severe I ever experienced) commenced yesterday morning at half-past eight, and lasted about a minute. I had just reached my house, and had barely time to station myself in the centre of the court-yard. The movement of the ground resembled a succession of waves, and I found it impossible to remove from the spot I occupied. My house has suffered no further damage than several cracks in the walls, and having the tiles that covered it completely jumbled together, and a number of them thrown down and broken. I am at this moment occupied in superintending repairs. Many of the inhabitants have left the city, and are living in the suburbs. We are still ignorant of what may have occurred in the other towns and villages. In addition to our calamities, the port of Guayaquil is still closely blockaded by the Peruvian squadron."

The following Communications were read :—

1. *Account of a Botanical Trip to Algeria.* By GEORGE S. LAWSON, Esq.

2. *Botanical Intelligence.* By Professor BALFOUR.

(1.) Biographical Sketch of Baron Von Humboldt, one of the foreign honorary members of the Society.

(See July No. of Journal, p. 168.)

(2.) Remarks on the Vitality of Seeds, particularly when subjected to the action of Sea-water.—The remarks had special reference to the experiments of M. Charles Martins of Montpellier. It has long been known that seeds are transported by oceanic currents. So far back as 1695, Sloane speaks of extraordinary beans being thrown by the sea on the shores of Scotland and of Ireland, and of which the inhabitants made snuff-boxes. One of the seeds he named *Phaseolus maximus perennis*, which may be recognised as *Mimosa scandens;* the second was *Dolichos urens;* and the third, *Guilandina Bonduc.* Sloane accounts for the transport of these seeds by the Gulf-stream and by westerly winds. Linnæus, in his travels along the northern coasts of Norway, tells us that the inhabitants of these regions found on the shores the seeds of *Cathartocarpus Fistula*, *Anacardium occidentale*, *Mimosa scandens*, and *Cocos nucifera.* M. Martins has gathered at North Cape, lat. 71° 12′ N., and long. 23° 30′ E., specimens of the seeds of *Entada Gigalobium*, D.C., or *Mimosa scandens*, L. The double coco-nut (*Lodoicea seychellarum*) is carried by a current from Praslin Island to the Maldives. If these currents are to contribute to the dissemination of seeds, it is of course necessary that the seeds should preserve their vitality,—*i.e.*, their germinating properties. Few experiments have been made on this subject, which is interesting not merely as regards the distribution of plants at the present epoch, but also as regards the distribution in fossil periods. During the Tertiary and other epochs, it is supposed that there was a vast expanse of ocean, and that the land consisted of a series of islands or archipelagos in the midst of it. In that case, currents must be reckoned as the chief agents in the dissemination of plants at these periods of the earth's history. If it is shown that currents could not contribute to this diffusion, then we must conclude that species separated by immense extents of oceans have not had a single centre of creation, but numerous centres. A single individual of each species could not be the common stock whence have arisen all the individuals existing on the surface of the earth. On the contrary, a certain number of individuals, specifically identical, must have appeared independently at different parts of the globe, very distant from each other. Necker affirms that the seeds thrown by the Gulf-stream on the coasts of Scotland do not germinate. Lyell however states, on the authority of Brown, that a plant of *Guilandina Bonduc* grew from a seed cast on the west coast of Ireland. Godron maintains that seeds immersed during the whole winter in pools of salt-water did not lose their vitality. Martins has seen seeds of *Cassia Fistula* germinate well, after being taken from fragments of the pods which had been thrown by the current on the coasts of Provence and Languedoc. These seeds had been protected by the pericarp, with its separate partitions. Salter also found grains of barley and oats, and various seeds, germinate after long submersion in sea-water. He says, in the year 1843 the authorities of Poole, in Dorsetshire, determined to deepen the channels of the Poole harbour to facilitate navigation. For this purpose a large number of ballast lighter barges were employed to scrape the mud from the bottom of the channel, and convey it to the shore, where it was deposited. The mud was collected in such quantity as to form a quay, which, however, was never used, nor was its surface disturbed. In the early spring abundant vegetation appeared on the quay, the plants being distinct from those of the neighbouring shores. The ordinary vegetation on the shores around was Statice, Salicornia, Salsola, Atriplex, Carices, &c., whilst on the mud there were *Lysimachia vulgaris*, *Centaurea calcitrapa*, and *Epilobium hirsutum*. The seeds of these plants, Mr Salter shows, must have been deposited in the mud of the harbour, and most probably have lain there for a long time. Charles Darwin called attention to the subject in 1856, and along with Berkeley he gave the results of his observations in the "Journal of Proc. of Linn. Soc. I.," p. 130, 1856. Darwin put seeds into small bottles filled with artificial sea-water; while Berkeley sent his to Mr Hoffman of Ramsgate, where they were exposed in baskets to the action of sea-water for about a month. Martins also carried on his experiments in 1856. His conclusions are as follows:—1. The greater number of seeds float in sea-water; about one-third sink instantly to the bottom. 2. One-third only of the seeds tried germinated after six weeks' immersion, and one-eleventh after three months. 3. Taking only into account the seeds which floated, the number of these which sprouted after six weeks' immersion is one-fifth of the whole, and after three months one-fourteenth. 4. Ranunculaceæ, Malvaceæ, Convolvulaceæ, seem least capable of resisting the action of sea-water. 5. Chenopodiaceæ, Polygonaceæ, Cruciferæ, Gramineæ, and Leguminosæ, resisted best prolonged immersion in the sea. 6. A hard perisperm and the presence of albumen seem to be favourable for the preservation of seeds. 7. The transport of seeds by currents seems to have had an insignificant share in the diffusion of species over countries separated by the sea; and if we consider the number of disjoined species which could only have been diffused in this way, the idea of numerous specific centres of creation acquires great probability.

3. *On the Flowering of a variety of* Cratægus oxyacantha *in the Edinburgh Botanic Garden.* By Mr James M'Nab.

During the past winter my attention was directed to a tree of the double-white flowering *Cratægus oxyacantha*, which retained most of its leaves in a green state during the whole winter, some of them still remaining on the tree even to this day, 12th May 1859. Early in February this tree presented a greenish appearance before all the other thorns, contrasting singularly with the old dark-green leaves. Owing to the comparatively mild weather we were then experiencing, the progress of leaf development was rapid. On the 14th of April the tree was covered with flower buds, having blossoms expanded on one small branch. At this time the tree is covered with flowers, but, strange to say, instead of being double, all are single, none of the blooms possessing more than five petals, and having the stamens and pistils perfectly normal. The tree has been grafted, and has been growing in its present situation for upwards of thirty years. It is 18 feet high, having a circular-shaped head 54 feet in circumference, with a stem 30 inches round. This thorn has flowered regularly during the month of June for many years; last year the flowering was particularly abundant, and remained long on the tree. Neither the leaves nor flowers are as large as usual, and the general health of the tree does not seem changed.

Thursday, 9th June.—ANDREW MURRAY, Esq., President, in the chair.

The following Communications were read :—

1. *Remarks on the Development of the Seed-vessel of* Caryophyllaceæ. By ALEXANDER DICKSON, Esq.

2. *Remarks on some Plants from Old Calabar.* By AND. MURRAY, Esq. (See July No. of Journal, p. 159.)

3. *Biography of Dr William Nichol.* By Professor BALFOUR.

Dr Balfour remarked—The painful duty devolves on me this evening of recording the death of Dr William Nichol, one of the Fellows of the Botanical Society. This melancholy event took place at Alexandria on the 7th of May 1859, while he was on his passage to India to join her Majesty's service there. Dr Nichol was the son of the late Walter Nichol, LL.D., for many years a successful teacher of mathematics in Edinburgh. I was one of his father's pupils. I am indebted to his friend Mr William Millar for some particulars in regard to the early life of Dr William Nichol. He was born in March 1836, and died, therefore, at the early age of twenty-three. He received his school education in the Edinburgh Institution, under the superintendence of Mr George Murray, a cousin of his father's. He was a diligent scholar, and gained prizes for Latin and Greek. From his earliest youth he evinced a desire for scientific knowledge, and received from his father encouragement in the prosecution of science. He pursued with much zeal mechanics and conchology, and acquired considerable proficiency in both. He also applied himself to the study of modern languages, and had a fair knowledge of German, French, and Italian. His taste, however, did not lie in the direction of languages, and he acquired them chiefly with the view of opening up means for the study of science. He always exhibited a remarkable desire to excel in anything to which he turned his mind. He became a student of the University of Edinburgh in 1850, at the age of sixteen. After attending the literary and mathematical classes, he commenced the study of medicine in the session 1853–54. I became acquainted with him in 1854, when he entered the botanical class. In that year he gained the first prize in the junior division of the class for excellency in the competitive examinations. He became ardently fond of botany, and directed his attention in a special manner to the more difficult department of cryptogamic botany. In muscology he became a proficient, and added many species of mosses to the flora of Scotland. He attended the botanical class again in 1855, and gained the highest prize in the senior division of the class. He also entered as a pupil in 1856. During these three years, as well as subsequently, I had ample opportunities of observing his zeal and enthusiasm, as well as his great power of diagnosis. I was satisfied that he was well fitted for occupying a situation of importance in the botanical world. Accordingly, after he had passed his examination for M.D. and surgeon in 1857, I recommended him to Sir Wm. Hooker for a botanical expedition. The situation, however, was filled up before Dr Nichol's application was received. By the advice of Sir William, Dr Nichol became a candidate for a medical appointment in the navy, and after passing his examination he was sent to Portsmouth. His health, however, began to suffer at this time, and he was compelled to give up work for a time. The unexpected death of his father afterwards led to an alteration of his plans, and he resigned his appointment in the navy. The death of a sister afterwards affected him much. These calamities seem to have operated painfully on his mind, and for a time he was unfit for much exertion; still he continued to prosecute his favourite studies in his own quiet way. His habits were naturally retiring, and his manner was such as not to secure for him at once that reception which his merits deserved. Had he possessed more confidence, and advanced his claims with greater vigour, he would have had great success. As his health and spirits improved, he became a candidate for an Indian medical appointment, and stood high on the list. He made preparations for his departure, and left Edinburgh about the beginning of April. At the time he left he had just recovered from an attack of gastric fever. This illness seems to have debilitated him much, and probably led to the fatal result. Dr Nichol sailed in the Pera, and by the time the vessel reached Alexandria he was dangerously ill. On the 5th of May Dr F. F. Ossler of Alexandria writes that he had been called to see him when he was landed from the Pera, and that he found him in a very precarious state indeed. All attention was paid to him by the surgeon of the vessel during the passage, as well as by Dr Ossler. He had suffered much from sea-sickness during the voyage, and he was brought on shore at Alexandria, labouring under typhoid fever, which carried him off in a few days, notwithstanding all the kind medical attention of Dr Ossler.

The Society agreed to record on their minutes their sense of the loss which they had sustained in the death of Dr Nichol, and to convey their sympathy to his afflicted mother.

4. *Remarks on the Temperature of Plants.* By M. Becquerel. (See July No. p. 156.)

Thursday, 14th July.—Andrew Murray, Esq., President, in the chair.

The following Communications were read:—

1. *Statistics of Botanical Class in the University of Edinburgh from 1777 till 1859 inclusive.* By Professor Balfour.

Date.	Medical.	General.	Winter.	Summer.	Total Students.
1777	—	—	—	—	54
1778	—	—	—	—	33

Date.	Medical.	General.	Winter.	Summer.	Total Students.
1779	—	—	—	—	45
1780	—	—	—	—	32
1781	—	—	—	—	58
1782	—	—	—	—	39
1783	—	—	—	—	52
1784	—	—	—	—	51
1785	—	—	—	—	67
1786	—	—	—	—	53
1787	—	—	—	—	48
1788	—	—	—	—	54
1789	—	—	—	—	54
1790	—	—	—	—	51
1791	—	—	—	—	71
1792	—	—	—	—	44
1793	—	—	—	—	58
1794	—	—	—	—	55
1795	—	—	—	—	59
1796	—	—	—	—	58
1797	—	—	—	—	76
1798	—	—	—	—	79
1799	—	—	—	—	82
1800	—	—	—	—	97
1801	—	—	—	—	106
1802	—	—	—	—	122
1803	—	—	—	—	106
1804	—	—	—	—	103
1805	—	—	—	—	75
1806	—	—	—	—	103
1807	—	—	—	—	115
1808	—	—	—	—	98
1809	—	—	—	—	127
1810	—	—	—	—	111
1811	—	—	—	—	132
1812	—	—	—	—	123
1813	—	—	—	—	136
1814	—	—	—	—	131
1815	—	—	—	—	140
1816	—	—	—	—	157
1817	—	—	—	—	180
1818	—	—	—	—	130
1819	—	—	—	—	124
1820	134	35	—	—	169
1821	171	30	—	—	201
1822	148	30	—	—	178
1823	162	25	—	—	187
1824	187	26	—	—	213
1825	217	31	—	—	248
1826	214	30	—	—	244
1827	181	19	—	—	200
1828	251	29	—	—	280
1828–29	220	38	37	219	258
1829–30	213	27	35	205	240
1830–31	221	22	38	205	243
1831–32	194	19	58	155	213
1832–33	229	31	46	214	260

Date.	Medical.	General.	Winter.	Summer.	Total Students.
1833–34	216	28	62	182	244
1834–35	213	26	42	197	239
1835-36	188	24	45	167	212
1836–37	188	39	38	189	227
1837–38	187	39	33	180	213
1838–39	146	17	24	139	163
1839–40	122	11	23	110	133
1840–41	101	18	24	95	119
1841–42	126	27	29	124	153
1842–43	113	23	26	110	136
1843–44	114	22	17	119	136
1845	106	17	—	—	123
1846	152	32	—	—	184
1847	145	54	—	—	199
1848	147	39	—	—	186
1849	148	36	—	—	184
1850	148	49	—	—	197
1851	169	37	—	—	206
1852	161	24	—	—	185
1853	194	39	—	—	233
1854	198	28	—	—	226
1855	163	26	—	—	199
1856	164	47	—	—	211
1857	155	37	—	—	192
1858	147	40	—	—	187
1859	211	42	—	—	253

2. *On the Fruits of the Cucurbitaceæ and Crescentiaceæ, as the original Models of various clay, glass, metallic, and other hollow or tubular vessels, and instruments employed in the Arts.* By Professor GEORGE WILSON.

The author commenced by drawing a contrast between the plan of procedure of an animal artist, such as a bird or an insect, and that which guides a human workman, when each is executing a design. Both classes of artists agree in eschewing chance, and following in their work a prearranged plan. They differ in this, that the human artist carries out his own conception, realising an idea which has arisen in his own mind, or at least a scheme which has been wrought out there; whilst the animal artist carries out a conception which is not its own, but a divinely-implanted instinct—in other words, a thought of God's. Each animal instinct is thus equivalent to an infallible recipe or formula of guidance, furnished by God to the creature which follows it; and the instinctive workmanship which results, for example, in the exquisite cell of the bee, the geometric web of the spider, the caterpillar's resurrection-shroud, and the swallow's nest, is as truly the copy of a God-given pattern as were the mercy-seat, or the golden candlestick of the Hebrew tabernacle which Moses constructed after the perfect models shown him on the Mount. However inferior, accordingly, the animal may be to the human workman, the work of the former, as an example of Divine design, may serve the latter better as a pattern for his own work than anything he could devise himself. A conviction of this kind has always dwelt in the minds of the intelligent men of all ages, and has induced them to admire and to copy both the workmanship and the organs or tools of the lower animals, as in many cases not admitting of being surpassed, or even equalled, by human devices. From the animal it is but a step to the plant; and if we miss consciousness, and cannot assure ourselves of even the smallest amount of intelligence, we are still certain of the presence of a manifold energetic subtile life, which gives to each vegetable organism powers of transforming matter still more marvellous in some respects than those of the animal. If, for example, the animal is more wonderful as a mechanician, the plant is more wonderful as a chemist. Nor do we fail to realise that the inner vegetable life which invests the plant with energies so far transcending those of the mineral on the one hand, and of the animal on the other, may be traced back from seedling to parent-plant through thousands of years; whilst we find for this life no beginning except in the will of God, and perceive that He is behind it, guiding and controlling it wherever it appears. The flowers, fruits, leaf-appendages, and other organs of plants which have in all ages been resorted to by mankind as patterns, are thus, though in a different sense, as truly divine patterns as the organs or workmanship of animals. Whilst thus to plants is given the unconscious realisation of Divine designs, and to animals the spirit of conscious and delightful obedience to perfect plans of work, to man has been granted the God-like power to create what in its degree shall bear his stamp as its maker, even as the universe shows everywhere the finger-touch of the Almighty. The human industrialist has thus plainly in his choice two modes of work: he may either fall back entirely upon his creative or inventive faculty, and devise novel instruments, or he may imitate, in whole or in part, vegetable or animal organisms (including animal workmanship), accepted as divine patterns ready to his hand. In reality, industrial man has constantly been following both modes, sometimes inventing, sometimes copying, often combining both. It is to certain of his *imitative* proceedings that reference will now be specially made; with, however, the proviso that those proceedings can in no case, from the difference of tool and material with which man works, be absolutely imitated, but only partly so. In endeavouring to ascertain what human instruments are copies of natural objects, we must not forget that certain forms are so common that mere similarity in shape, between a particular industrial instrument or vessel, and a natural object, is not enough to show that the one has been imitated from the other. Thus, a very familiar form of wine-cup recalls closely the configuration of a truncated polished coco-nut, but not more than it does an ostrich-egg similarly treated, or the lower part of a bottle-gourd. It also resembles the capsule of some species of poppy, and also the *theca* or spore-case of certain mosses; whilst the calyx of the rose gives the perfect contour of our wine-cup. It might then have been modelled after any one of those patterns, and it may have been after none of them, for among the ovals of the mathematician would be found outlines which would exactly give the form we have been describing. Similarity, then, or even identity, of form, especially in the case of simple shapes, is no certain proof of the one having been copied from the other, but it is an important guide to the derivation of an artificial shape from a natural one. In seeking for further means of certifying the relation of similar forms to each other, it will be found that the name by which an industrial instrument or vessel is known often points to the form or material of its prototype, which it has long since ceased to resemble in most particulars. Thus, the brewer offers his visitor a *horn* of ale. If the offer is accepted, the *horn* makes its appearance in the shape of a tin mug, still indicating by its shape and name that the original drinking-vessel was made from the wider part of a cow's horn. A hunting-*horn* again is now a brass trumpet, which, in its more elaborate forms, completely conceals from us that it is but a lengthened many-coiled and very wide-mouthed horn of brass. So, also, a shoe-*horn* is often a scoop of metal. We have in the same way the Latin name *tibia* applied to a musical pipe or flute, preserving the fact that it was originally made from the leg-bone of an animal; and the name *avena*, applied to the pastoral pipe, recalling that it was at first a straw or reed. So also the Greek *alabastron*, primarily limited to a perfume-box made of alabaster, came latterly among the ancient Orientals to signify any kind of box employed to contain a fragrant oil or ointment of high price. In addition to the name, we have an important means of identifying the originals of modern and familiar vessels which have been long in use among highly civilised nations, in observing the mode in which nations of another type of civilisation than ourselves, or little advanced in civilisation at all, supply the absence of such vessels as we use. Keeping those points in view, it would appear that our modern hollow and tubular vessels have been mainly modelled on three distinct types:—Firstly, the stems, leaves, flowers, and fruits of plants; secondly, the bones, including the hollow horns of the mammalia; thirdly, the shells or external skeletons of the mollusca. Omitting all reference to the last two heads, and limiting attention solely to fruits under the first, the relations of the Cucurbitaceæ and Crescentiaceæ may now be considered. No tribe of plants appears to have yielded so many different hollow vessels as the *Cucurbitaceæ* or Gourd Family, with which, however, it will be convenient to include the *Crescentiaceæ* or calabash-trees yielding the calabash. This has apparently arisen—1st, from their wide distribution over the globe, which is so extensive that in all its hotter regions members of this family are indigenous, and they are easily cultivated in the warmer temperate regions; 2d, they yield fruits of great diversity of form, but possessed in certain varieties of firm, hard rinds; 3d, they are easily deprived of their pulp and rendered hollow; 4th, they can, whilst growing, be to some extent moulded in form, and though their rinds when dry are hard, they are easily cut and perforated. Next to the hollow horn of the mammalia, the gourd, representing all the Cucurbitaceæ and the calabashes, yields the greatest variety of vessels. In illustration, the following examples, along with others bearing on different references in the paper, were laid on the table, the specimens being taken from the Botanical and Industrial Museums:—An elaborately carved ovoid work-box; a basket with and one without a handle; various South and West African snuff-bottles or boxes; plates, plate-covers; cups; bowls; water-jug; water jar; cooking pot; spoon or ladle; scoop or skimmer; vessel, with suction-tube and strainer, answering as teapot; cup and saucer in one (used by South American Indians for maté tea); funnel used for medical purposes by Africans at Old Calabar; flask-like bottle; constricted or hour-glass; pilgrim's bottle; cupping instrument, as used at Old Calabar. Besides these, reference was made, on the authority of Dr A. Hunter of Madras, to the construction by the natives of India of a musical instrument resembling the guitar from a large flask-like gourd; and to their conversion of a similar but much smaller gourd into a suckling-bottle for children. Allusion was also made, on the authority of the same gentleman, and also on that of Mr. A. Hewan, of the Old Calabar mission, to the employment in India and Africa of empty calabashes as floats, or the components of rafts, by means of which men, animals, and baggage were ferried across rivers. A single calabash, it was stated, was quite sufficient to float a man, and the Africans contrived to balance themselves on one in a very adroit, but to white men inimitable way. The author further reminded his audience that, according to an authority in whom they had much faith in their early days, Cinderella's friend the Fairy made her a splendid coach out of a pumpkin, or immense gourd. He was afraid this was a lost art, at least at this season. It can apparently be practised only at Christmas-time, when the pantomime is in vogue. Nevertheless, it might be that Cinderella's pleasant story contains, besides much else, a recognition of the manifold uses to which a gourd may be industrially applied. Without insisting on this, the gourd might with confidence be regarded as having been in both hemispheres the precursor and natural model of the cup or bowl afterwards fashioned in clay and metal. In particular, however, it was the prototype of three hollow vessels. The first is the long-necked bottle, with an egg-shaped body, which has been imitated in clay from a Cucurbitaceous fruit by the Chinese, the natives of India, the ancient Egyptians, and various African tribes, as well as by the ancient Persians, to mention no others. The similarity of the clay bottles of these nations to gourds has struck all observers; and the botanist, by the term "bottle-gourd" applied to the fruit of one particular plant, has implied his recognition of that species, as the best marked vegetable prototype of the artificial liquor-flask. Recently Minton has produced a graceful copy of the bottle-gourd, in yellow majolica ware, with curling tendrils and leaves in green, as if in the name of the Ceramic art to do homage to a natural model which it has followed in all countries for thousands of years. From clay the transition is ready to glass, in which the bottle-gourd, either at first or second hand, has been constantly imitated. Secondly, the constricted or hour-glass gourd is the accepted or traditional pilgrim's bottle, drawn by our artists as slung by its narrow waist over the shoulder of the wayfarer in their illustrated "Pilgrim's Progress" and "Palmer's Crusading Journeys." The gourd is supposed by some to owe its constriction and hour-glass form to the effect of a ligature tied round it while growing; but such a process is certainly not necessary. Plants grown abroad under the eyes of botanists, as well as several raised in the Botanic Garden here by Mr M'Nab, spontaneously produced fruit with the useful constriction which enables it to be hung by a string across the shoulder. In Europe this form of bottle is now little seen, but it is otherwise in the East, where it is copied in clay and porcelain as a convenient water-vessel. For actual pilgrims, the hollow gourd itself has the advantage of being less fragile than its clay copies. Both, however, are used; in illustration of which two small procelain figures of Chinese wandering beggars were shown, each with his hour-glass gourd or its fac-simile in clay. The third, and perhaps the most curious, as certainly it is the most novel, relation of the gourd to hollow vessels, is as the prototype of the cupping-glass. The author was led to suspect this in consequence of receiving a year ago the gift of a small gourd for the Industrial Museum. It had been sent to Dr Greville by Mr Hewan, the mission surgeon at Old Calabar, and was stated to have been used by the natives for cupping. The author was led to recall the fact that the Latin name for the cupping-glass was "cucurbitula cruenta," and to surmise that the name was a standing memorial of the earliest cupping instrument having been a gourd. This summer he had the opportunity of meeting Mr Hewan, who is on a visit to this country, and from him he learned that the practice of cupping with gourds is common in Western Africa, as well as among the negroes in Jamaica. Lexicographers have explained the Latin name of the cupping-glass as referring to the resemblance in *shape* of the latter to a gourd; but it does not more resemble a gourd than various other objects. Moreover, a cupping instrument as ancient as the "cucurbitula" was the tip of a cow's horn, perforated at the apex, so as to allow the air to be sucked out by the lips after the base had been applied to the scarified skin; yet no two shapes are more dissimilar than those of the egg-like gourd and the conical horn. The author accordingly infers that the Latin cucurbita or *cucurbitula,* and the Greek *Sicyos* were alike applied to the cupping-glass mainly because originally it was a gourd, although unquestionably the permanency of the term was secured by the instrument retaining the gourd form after it parted with the gourd substance. The gourd was thus in a twofold way the prototype of the cupping instrument. In illustration of these points the author showed three African cupping-gourds; a Shetland "Blude horn," or cupping-horn, as used at the present day; and drawings of the gourd-shaped bronze *Cucurbitulæ* found in the ruins of Herculaneum and Pompeii. A brief allusion was made to the cupping-gourd and cupping-horn, as the earliest artificial vacuum-producers and precursors of the air-pump; but this subject was reserved for a special investigation.

Mr M'Nab placed on the table a series of living Alpine Plants, including fifteen or twenty collected by Dr Balfour's party in Switzerland; also a specimen of *Sabbatia stellaris,* showing a peculiarity in the style, which lies at first flat on the corolla with the two stigmas close to each other, and after the pollen is applied becomes erect, with the stigmas separate. He likewise exhibited specimens of *Helleborus niger,* or Christmas Rose, in full flower; also a hawthorn branch, with the fruit of last year and of the present year attached to it.

Mr Hewan exhibited a specimen of the flower of *Aristolochia gigantea* from Africa.

Antiquarian Society of London.

June 2.—O. Morgan, Esq., V.P., in the chair.

Mr Evans read a paper "On the occurrence of Flint Implements in undisturbed beds of gravel, sand, and clay (such as are known by Geologists under the name of Drift) in several localities, both on the continent and this country."—The first discovery of these implements is due to M. Boucher de Perthes, of Abbeville, who, in the pits in that neighbourhood, found flint evidently fashioned by the hand of man, under such conditions as forced upon him the conclusion that they must have been deposited in the spots where they were found at the very period of the formation of the containing beds. M. de Perthes announced his discoveries in a work entitled "Antiquités Celtiques et Antédiluviennes," in 2 vols., the first published in 1849, and the second in 1857; but owing in some measure to the admixture of theory with the facts therein stated, his work has not received the attention it deserves. The late discovery in the Brixham Cave, in Devonshire, of flint weapons in conjunction with the bones of the extinct mammals, had brought the question of the co-existence of man with them again prominently forward among geologists, and determined Mr Prestwich, who has devoted much attention to the later geological formations, to proceed to Abbeville, and investigate upon the spot the discoveries of M. de Perthes. He had there been joined by Mr Evans, and they had together visited the pits where flint weapons had been alleged to have been found, both in the neighbourhood of Abbeville and Amiens. The chalk hills near both these towns are capped with drift, which apparently is continued down into the valleys, where it assumes a more arenaceous character, and in these beds of sand, as well as more rarely in the more gravelly beds. Upon the hills, mammalian remains have been found in large quantities. They include the extinct elephant, rhinoceros, bear, hyena, tiger, stag, ox, and horse—in fact, most of the animals whose bones are so commonly associated together in the drift and caverns of the Postpliocene period. On the hills near Abbeville, and at St Acheul, near Amiens, the drift varies in thickness from about ten to twenty feet, and consists of beds of subangular gravel, with large flints, and above them sands containing the fragile shells of fresh-water mollusca, and beds of brick-earth. It is among the basement beds of gravel, at a slight distance above the chalk, that the flint implements are usually found. They are of three forms:—1. Flakes of flint apparently intended for knives or arrow-heads. 2. Pointed implements, usually truncated at the base, and varying in length from four to nine inches—possibly used as spear or lance heads, which in shape they resemble. 3. Oval or almond-shaped implements, from two to nine inches in length, and with a cutting edge all round. They have generally one end more sharply curved than the other, and occasionally even pointed, and were possibly used as sling-stones, or as axes, cutting at either end, with a handle bound round the centre. The evidence derived from the implements of the first form is not of much weight, on account of the extreme simplicity of the implements, which at times renders it difficult to determine whether they are produced by art or by natural causes. This simplicity of form would also prevent the flint flakes made at the earliest period from being distinguishable from those of a later date. The case is different with the other two forms of implements, of which numerous specimens were exhibited; all indisputably worked by the hand of man, and not indebted for their shape to any natural configuration or peculiar fracture of the flint. They present no analogy in form to the well-known implements of the so-called Celtic or stone period, which, moreover, have for the most part some portion, if not the whole, of their surface ground or polished, and are frequently made from other stones than flint. Those from the drift are, on the contrary, never ground, and are exclusively of flint. They have, indeed, every appearance of having been fabricated by another race of men, who, from the fact that the Celtic stone weapons have been found in the superficial soil above the drift containing these ruder weapons, as well as from other considerations, must have inhabited this region of the globe at a period anterior to its so-called Celtic occupation. This difference in form and character from the ordinary types of stone implements strengthened the probability of their having been found under entirely different circumstances; and Mr Evans then proceeded to examine the evidence of their having been really discovered in undisturbed beds of gravel, sand, and clay. He showed from various circumstances in connection with them, such as their discoloration by contact with ochreous matter, whitening when imbedded in a clayey matrix, and in some instances being incrusted with carbonate of lime, the extreme probability of their having been deposited in these beds at the very time of their formation, inasmuch as the unwrought flints adjacent to them had been affected in a precisely similar manner, and to no greater extent. This discoloration and incrustation of the implements also proved that they had really been found in the beds out of which they were asserted to have been dug; and their number and the depth from the surface at which they were found were such, that if they had been buried at any period subsequent to the formation of the drift, some evident traces must have been left of the holes dug for this purpose; but none such had been observed, though many hundreds of the implements had been found dispersed through the mass. But besides this circumstantial evidence, there was the direct testimony of MM. Boucher de Perthes, Rigollot, and others, to the fact of these implements having been discovered underneath undisturbed beds of drift, and many of them under the immediate eye of M. de Perthes, who, indeed, had been the first to point out the existence of these implements to the workmen. Of the correctness of this testimony, the writer, when visiting with Mr Prestwich the gravel pit at St Acheul, near Amiens, had received ocular proof. There, at the depth of eleven feet from the surface, in the face of the bank or wall of gravel, the whole of which, with the exception of the surface soil, had its layers of sand, gravel, and clay entirely undisturbed, was one of these implements *in situ*, with only the edge exposed, the remainder being still firmly imbedded in the gravel. After having photographs taken of it, so as to verify its position, this implement had been exhumed, and was now exhibited with other specimens. At a subsequent visit of Mr Prestwich and some other geologists to the spot, one of the party, by digging into the bank of gravel at a depth of sixteen feet from the surface, had dislodged a remarkably fine weapon of the oval form, the beds above being also in a perfectly undisturbed condition. The inevitable conclusion drawn from these facts was, that M. Boucher de Perthes' assertions were fully sub stantiated, and that these implements had been deposited among the gravel at the time of the formation of the drift. And this conclusion was corroborated in the most remarkable manner by discoveries which had been made long since in England, but whose bearing upon this question had, until the present time, been overlooked. In the 13th volume of the "Archæologia," is an account by Mr Frere, in 1797, of the discovery of some flint weapons in Suffolk, in conjunction with elephant remains, at a depth of eleven to twelve feet from the surface, in gravel overlaid by sand and brick-earth, presenting a section extremely analogous with some that might be found near Amiens or Abbeville. Some of these weapons are preserved in the Museum of the Society of Antiquaries and in the British Museum, and are identical in form with those found on the Continent. Mr Prestwich had been to Suffolk, and verified the discoveries recorded by Mr Frere. Flint implements are still found there, as well as mammalian remains, but in diminished quantity, only two of the weapons having been brought to light during last winter. Another of these implements is in the British Museum, having been formerly in the Kemp and Sloane collections, and is recorded to have been found with an elephant's tooth in Gray's Inn Lane. Similar implements are also reported to have been found in the gravel near Peterborough. These accumulated facts prove, almost beyond controversy, the simultaneous deposition of instruments worked by the hand of man with bones of the extinct mammalia in the drift of the Postpliocene period. Whether the age of man's existence upon the earth is to be carried back far beyond even Egyptian or Chinese chronology, or that of the extinct elephant, rhinoceros, and other animals brought down nearer to the present time than has commonly been allowed, must remain a matter for conjecture. Thus much appears nearly indisputable:—That at a remote period, possibly before the separation of England from the Continent, this portion of the globe was densely peopled by man; that implements, the work of his hands, were caught up together with the bones of the extinct mammals by the rush of water through whose agency the gravel beds were formed; that above this gravel, in comparatively tranquil fresh-water, thick beds of sand and loam were deposited, full of the delicate shells of fresh-water mollusca; and that where all this took place now forms tableland on the summit of hills nearly 200 feet above the level of the sea, in a country whose level is now stationary, and the face of which has remained unaltered during the whole period which history or tradition embraces. In conclusion, Mr Evans suggested a careful examination of all beds of drift in which elephant remains had been found, with a view of ascertaining the co-existence with them of these flint implements, and still farther illustrating their history. Their rudeness, and the fact that they had not been sought for by those who have investigated the drift, may well account for their not having been more generally found. He mentioned the banks of the Thames, the eastern coast of England, the western coast of Sussex, the valleys of the Avon, Severn, and Ouse, as localities where the existence of the mammaliferous drift was well known, and where there was every probability that a search for these implements, the earliest records of the human race, would be rewarded by success.

Abstracts of the Proceedings of the Geological Society of London.

April 6, 1859.—Professor J. Phillips, President, in the Chair.

The following Communication was read :—

On the Subdivisions of the Inferior Oolite in the South of England, compared with the Equivalent Beds of the same Formation on the Yorkshire Coast. By Thomas Wright, M.D., F.R.S.E. Communicated by T. H. Huxley, Esq., Sec. G.S. With a Note on Dundry Hill, by R. Etheridge, Esq., F.G.S.

The author first remarked that, since the publication of his memoir "On the so-called Sands of the Inferior Oolite" in the Society's Journal (vol. xii., p. 292), some geologists, both in England and on the Continent, had taken the Liassic character of these sands into consideration, and that Oppel, Hébert, Marcou, and Dewalque had agreed with the author on palæontological grounds; whilst in England, Mr E. Hull (of the Geological Survey) had also adopted his views. On the other hand, in recent memoirs, Mr Lycett regards them as forming a distinct stage, and Professor Buckman still retains them in the Inferior Oolite.

Dr Wright then described the beds at Bluewick, on the Yorkshire coast, which he regards as the equivalents of the "Cephalopoda-bed" or "Jurensis-bed;" namely, some shales and sandstones underlying the rock, which he considers to be the basement bed of the "Dogger" or Inferior Oolite.

These are—1. (uppermost) Shales with *Terebratula trilineata*, *Belemnites compressus*, *B. irregularis*, and *Trigonia Ramsayi*. 2. Sandstone, yellow, with *Turritella*, *Trigonia*, *Astarte*, *Ammonites concavus*, *A. variabilis*, &c. 3. Yellow Sandstone or Serpula-bed. 4. Grey Sandstone or Lingula-bed, with *Lingula Beanii*, *Orbiculà*, *Belemnites compressus*, *B. irregularis*, *Ammonites Moorei*, &c.

The author then observed that the Inferior Oolite in the South of England admits of a palæontological subdivision into three zones, having the Fuller's-earth with *Ostrea acuminata* above, and the Cephalopoda-bed with *Ammonites opalinus* beneath :—1st (uppermost), the zone of *Ammonites Parkinsoni*; 2d, zone of *Am. Humphriesianus*; and 3d, zone of *Am. Murchisonæ*. He then described the lowest of these zones, that of *Am. Murchisonæ*, giving as synonyms "Dogger" (part), Young and Bird, and Phillips; "the central and lower division of the Inferior Oolite," Murchison; "Fimbria-stage of the Inferior Oolite," Lycett; "Brauner Jura β," Quenstedt; "Calcaire lædonien" (part), Marcou; "Calcaire à entroques," Cotteau; "die Schichten des Am. Murchisonæ," Oppel. The Leckhampton section was then described, as illustrating this zone, which was also described in its details as seen at Crickley Hill, near Cheltenham, and at Beacon Hill; also at Frocester Hill and Wooton-under-Edge.

The preceding sections exhibit the lithological character and stratigraphical relations of the Pea-grit and Freestones, which, however, undergo great and very important modifications when examined over even a limited area,—the Pea-grit as regards its structure; and the Freestone, its thickness. In the Southern Cotteswolds the Pea-grit loses its pisolitic character; and in the eastern part of the hill district the Freestones thin out and finally disappear; the Inferior Oolite being represented at Stow-on-the-Wold and at Burford by the zone of *Ammonites Parkinsoni*, with its light-coloured ragstones, filled with an abundance of *Clypeus Plotii*, Klein, and forming a "*Clypeus-grit.*"

The fossils of the Pea-grit and Freestone, and of the Oolite-marl or Fimbria-bed, were then enumerated. The Oolite-marl was described as having been probably derived from the debris of a coral reef: its Nerinæan limestone was particularly alluded to.

The section at the Peak near Robinhood's Bay afforded the author the equivalents of the zones of *Am. Humphriesianus* and *Am. Murchisonæ*, and was described in full.

The zone of *Am. Humphriesianus* was next treated of. Its synonyms are "Inferior Oolite of Dundry Hill," Conybeare and Phillips; "Grey limestone, Bath or Great Oolite" (Yorkshire), Phillips; "Eisenrogenstein (part) und Walk-Erde Gruppe," Fromherz; "Brauner Jura γ und δ," Quenstedt; "Calcaire ferrugineux," Terquem; "Blaue Kalke, Korallenschicht, Giganteus-Thone, und Ostreen-Kalke" (Quenstedt), Pfizenmayer. The best types of this zone, so well characterized by peculiar Gasteropods and Cephalopods, and its ferruginous oolitic grains, are seen in the section at Dundry Hill, at Yeovil and Sherbourne in Somerset, and at Burton-Bradstock and Chideock in Dorset. Just as the thinning-out of the Murchisonæ-zone and the absence of the Humphriesianus-zone near Burford and other localities in the N.E. parts of the Northleach district brings the Parkinsoni-zone nearly into juxtaposition with the clays of the Upper Lias, so the thinning-out of the Murchisonæ-zone at Dundry Hill brings the zone of *Am. Humphriesianus* into close relation with the "Sands of the Upper Lias," and has caused it to be mistaken for the "Cephalopoda-bed" of Frocester and Leckhampton Hills. In the Northern Cotteswolds the Humphriesianus-zone is but feebly represented.

The Dundry Hill section was then described in a note by Mr R. Etheridge, F.G.S., as comprising—1st (lowest), Lower Lias; 2d, perhaps the "Lias Sands;" 3d, the Shell-bed; 4th, Ammonite-bed (not equivalent to the "Cephalopoda-bed" of the Cotteswolds); 5th to 9th, shelly beds, ragstone, fine-grained oolite, and freestone; some of the latter representing the Parkinsoni-zone.

Dr Wright then described the section in Gristhorpe Bay, from the Cornbrash to the Millepore-bed;—equal to the zone of *Am. Humphriesianus*. The fossils of these marine and fresh-water beds were noted as existing in the cabinets of Leckenby, Bean, and others.

The zone of *Am. Parkinsoni* has the following synonyms, according to the author:—"Trigonia-grit and Gryphite-grit," Murchison and Strickland; "Ragstone and Clypeus-grit," Hull; "Spinosa-stage," Lycett; "Brauner Jura ε" (pars), Quenstedt; "Parkinsonthone, Brauner Jura δ und ε" (pars), Pfizenmayer; "Calcaire à Polypiers," Terquem; "Die Schichten des Ammonites Parkinsoni," Oppel. This zone is the most persistent of the three subdivisions of the Inferior Oolite, and is its only representative in the south-eastern parts of Gloucestershire.

The sections of Leckhampton Hill, Ravensgate Hill, Cold Comfort, Birdlip Hill, and Rodborough Hill, afford the fossils and details illustrative of this zone.

In this communication Dr Wright endeavoured to show that the Inferior Oolite of the South of England admits of a subdivision into three zones of life, and that each zone is characterized by the presence of Mollusca, Echinodermata, and Corals special to each. 2d. That these three zones are very unequally developed in different regions both in England, France, and Germany, the individual beds composing the zones being sometimes thin and feebly developed (or altogether absent) in some localities, but thick and fully developed in others; the zone of *Am. Murchisonæ* is the one most frequently absent; that of *Am. Humphriesianus* has a wider area; and the zone of *Am. Parkinsoni* is the most persistent, is widely extended, and is very often the sole representative member of the Inferior Oolite formation. 3d. That many *Lamellibranchiata* and a few *Gasteropoda* are common to the three zones, and that most of the *Ammonites*, *Brachiopoda*, *Echinodermata*, and Corals, are limited in their range to one of the zones; but that each zone possesses a fauna which is sufficiently characteristic of it. 4th. The Parkinsoni-zone possesses many species of *Mollusca* and *Echinodermata* in common with the Cornbrash; and the Murchisonæ-zone, in like manner, contains many *Lamellibranchiata*, which appeared for the first time in the Jurensis-stage, although all the *Cephalopoda* of these two stages are specifically distinct from each other.

April 20, 1859.—Major-General Portlock, V.P., in the Chair.

The following Communications were read :—

1. *On some Reptilian Remains from South Africa.* By Prof. Owen, F.R.S., F.G.S.

Fam. Crocodilia. *Galesaurus planiceps*, the Flat-headed Galesaur (from γαλῆ, polecat, σαῦρος, lizard), a genus and species founded on an entire cranium and lower jaw. The skull in length less than twice the breadth, much depressed, and flat above. Occipital region sloping from above backward, divided by a high and sharp ridge from the temporal fossæ; these are wide and rhomboidal; orbits small; nostril single and terminal. Dentition, $i.\ \frac{4-4}{3-3}$, $c.\ \frac{1-1}{1-1}$, $m.\ \frac{11-11}{12-12}$; all the teeth close-set, except the intervals for the crowns of the long canines when the mouth is closed. Canines of the shape and proportions of those in *Mustela* and *Viverra*, without trace of preparation of successors in the sockets; of quite mammalian character. Incisors longish and slender, molars subcompressed; both with simple pointed crowns, of equal length, and undivided roots. Original transmitted to the British Museum by Governor Sir George Grey, K.C.B. From the sandstone rocks, Rhenosterberg.

Cynochampsa laniarius, the Dog-toothed Gavial (from κύων, dog, and χάμψαι, Egyptian name for Crocodiles, applied by Wagner to the Indian Gavial). This genus and species is founded on the rostral end of the upper and lower jaws of a Crocodilian Reptile, with a single terminal nostril, situated and shaped as in *Teleosaurus*, and indicating similarly long and slender jaws. Only the incisive and canine parts of the dentition are preserved; but these closely correspond with the same parts in *Galesaurus*, the incisors being equal and close-set, of simple conical form, and the canines suddenly contrasted by their large size. In shape they resemble closely the completely formed canines in Carnivorous Mammals. There is no trace of successional teeth. Original transmitted to the British Museum by Governor Sir George Grey, K.C.B., from Rhenosterberg, South Africa.

Fam. Dicynodontia. Subgenus *Ptychognathus*, Ow. (πτύχος, ridge, γνάθος, jaw).—This subgenus is founded on four more or less entire skulls, two retaining the lower jaw, referable to two species.

Ptychognathus declivis, Ow.—Plane of occiput meeting the upper (fronto-parietal) plane at an acute angle, rising from below upward and backward, as in the feline mammals; fronto-parietal plane bounded by an anterior ridge, extending from one superorbital process to the other; from this ridge the facial part of the skull slopes downward in a straight line, slightly diverging from the parallel of the occipital plane (superoccipital ridge much produced and notched in the middle; the occipital plane, owing to the outward expansion of the mastoid plates, is the broadest part of the skull, which quickly contracts forward to the rigid beginnings of the alvéoli of the canine tusks; orbits oblong, reniform, suggestive of the reptile having the power of turning the eyeball, so as to look upward and backward as well as outward. Remains of sclerotic plates. Nostrils divided by a broad, flat, upward production of premaxillary, situated nearer the orbit than the muzzle, smaller than in type *Dicynodon;* temporal fossæ broader than long, and with the outer border longest; palate with single large oval vacuity, bounded by palato-pterygoid ridges: occipital hypapophyses proportionally thicker than in *Dicynodon tigriceps;* no trace of median suture in parietal, which is perforated by a "foramen parietale;" frontals divided by a median suture, and support a transverse pair of small tuberosities; anterior boundary-ridge of vertex formed by the nasals and prefrontals, the outer surface of both being divided into a horizontal and sloping facet; lacrymal bone extending from fore-part of orbit half an inch upon the face to the nostril; premaxillary long and single, its median facial tract flat, with a low median longitudinal ridge; maxillaries forming the lower boundary of the nostrils, and uniting above with the prefrontal, lacrymal, and nasal bones, their outer surface divided by the strong ridge suggesting the subgeneric name; teeth of the upper jaw restricted to the two canine tusks, the sockets of which descend much below the edentulous alveolar border; lower jaw edentulous, deep, and broad, with the fore-part of the symphysis produced and bent up to meet the seemingly truncate end of the premaxillary,—a character indicating, with the angular outline of the skull, the subgeneric distinction.

Ptychognathus verticalis.—The skull of this species, repeating the subgeneric characteristics of the foregoing, has the facial contour descending almost vertically from, and at almost a right angle with, the fronto-parietal plane. Orbits proportionally larger and more fully oval. Ridged sockets of the canine tusks descending more vertically from below the orbits. Originals transmitted to the British Museum by Governor Sir George Grey, K.C.B., from Rhenosterberg, South Africa.

Subgenus *Oudenodon*, Bain (οὐδείς, none, ὀδοὺς, tooth).—The skull in this subgenus presents the divided nostrils, the structure and the rounded contours, of that of the true *Dicynodons;* also the same form, relative size, and position of the orbits and nostrils; but the zygomatic arches are more slender, straight, and long; and, although there be an indication of an alvéolar process of the superior maxillary, the lower part of which projects slightly beyond the rest of the edentulous border of the jaw, it does not contain any trace of a tooth, so that both jaws are edentulous,—a character which had attracted the attention of their discoverer, Mr Bain, who, in indicating it, proposed the name *Oudenodon.*

It is permissible to speculate on the possibility of these toothless Dicynodontoids being, after the analogy of the Narwhals, the females; or of their being individuals which had lost their tusks without power of replacing them, as the known structure of the true Dicynodons indicates. But there are characters of the zygomatic arches and temporal fossæ which differenciate the toothless skulls sufficiently to justify their provisional reference to a distinct subgenus.

Hyoid apparatus of *Oudenodon.*—Beneath one of the skulls, and imbedded in the matrix between the mandibular rami, were the following elements of the hyoid apparatus:—basi-hyal, cerato-hyals, thyro-hyals (or hypo-branchials), cerato-branchials, and uro-hyal.

The cerato-hyals are long, subcompressed, expanded at both ends; the thyro-hyals shorter and more slender; the cerato-branchials with a sigmoid flexure; the uro-hyal, symmetrical, broad, flat, semicircular, with a production like a stem from the middle of the anterior margin. This apparatus shows the complexity by which the hyoid in Lizards and Chelonians differs from the hyoid in Crocodiles, and combines Chelonian with Lacertian characters. Transmitted by Mr Bain from South Africa.

Dicynodon tigriceps.—Pelvis: ilium, ischium, and pubis coalesced to form an "os innominatum," with the suture at the symphysis obliterated. At least five sacral vertebræ; the first with broad, thick, triangular, terminally-expanded pleurapophyses. The strong, straight, trihedral ilium overlies the above sacral rib, and extends forward to overlie also the last long and slender rib of the free trunk (thoracic) vertebræ. There are no lumbar vertebræ.

Pubis very thick, strong, with a broad anterior convexity resembling that of the *Monitor* in its internal perforation and external apophysis; ischium receiving the abutment of the last two pairs of sacral vertebræ.

The form of the anterior aperture of the pelvis is oval, with the sides broken by a slight angle at the middle, and the small end encroached upon by the slight angular prominence of the symphysis pubis. The long diameter is 11 inches (from the fore-end of the first sacral vertebra), the transverse diameter is 10 inches. The fore-half of this aperture is bounded by the first sacral vertebra exclusively, at the middle by its centrum, at the sides by its ribs; the hind-half of the aperture is bounded by the pubic bones. From the penultimate sacral vertebra to the symphysis pubis, it measures 5 inches.

The outlet of the pelvis is of a semi-elliptic form, 9 inches in transverse, and 4 inches in the opposite diameter. Original transmitted by Mr Bain from East Brink River, South Africa.

CROCODILIA (?). Genus *Massospondylus*, Ow. (Gr. μάσσων, longer, σπόνδυλος, vertebra.—The author exhibited diagrams, and pointed out the characters on which he had founded (in the Catalogue of Fossil Remains of the Museum of the College of Surgeons) the genus *Massospondylus*, exemplified by the *M. carinatus*.

Genus *Pachyspondylus*, Ow. (Gr. παχὺς, thick; σπόνδυλος, vertebra).—The fossils exemplifying this genus form part of the same collection, obtained by Messrs Orpen from sandstones of the Drakenberg range of hills, South Africa, and presented to the College of Surgeons.

2. *On the South-easterly Attenuation of the Lower Secondary Rocks of England, and the probable depth of the Coal-formation under Oxford and Northamptonshire.* By EDWARD HULL, Esq., A.B., F.G.S.

By a series of comparative sections, made by actual admeasurements by the officers of the Geological Survey, it was shown that all the Lower Secondary formations attain their greatest development towards the north-west of England; and, on the other hand, they become attenuated, and in some cases actually die out, in the opposite direction. For example, it was shown that the Bunter Sandstone in Cheshire reaches a thickness of 2000 feet, in Staffordshire 600, and in East Warwickshire is absent; and a similar law of south-easterly attenuation was shown to maintain in the case of the Keuper, Lias, Inferior Oolite, and lower zone of the Great Oolite.

It was shown that the upper zone of the Great Oolite (the White and Grey Limestones of Wilts, Oxford, Lincoln, and Yorkshire) forms the first exception to the law; and from the fact of its occurrence in the Bas-Boulonnais below the Chalk, and resting on carboniferous rocks, the author inferred that it extends more or less uninterruptedly from England to France and Belgium, and southward to Mr Godwin-Austen's palæozoic axis. The cause of this superior degree of persistency was referred to the organic, as distinct from the sedimentary nature of the formation, and its accumulation (like the White Chalk) on a deep sea-bed by the agency of Molluscs, Corals, and Foraminifera.

It was shown that the Lower Permian beds are scarcely represented in Lancashire and North Cheshire, but that they attain their greatest development (1800 feet) along a band of country stretching west and east from Salop to Warwickshire; and the author traced the margin of the basin in which they were formed, along the west, north, and east. The *local* origin of these Permian beds, as having been derived from the Old Red and Silurian lands by which they were surrounded, was insisted upon, and especially as agreeing with the observations of Murchison, Ramsay, and other authors.

As contrasted with this local origin of the Lower Permian Rocks of Central England, it was shown that the sedimentary materials of which the Triassac Rocks are formed must have been drifted by an ancient oceanic current from a continent or large tract of land occupying the position of the North Atlantic, and that the sediment was spread over the plains of England as long as it was mechanically suspended. The increasing distance towards the south-east from the source of supply accounted for the tailing-out of the sediment. During the Bunter Sandstone period, this sediment was drifted through the channel formed by the great headlands of Westmoreland and North Wales; but, as the whole area was gradually sinking (with occasional interruptions) during the periods of the Upper Trias and succeeding formations, the Welsh and Cumbrian mountains must have been nearly covered by sea at the close of the Liassic period.

The author adduced the following reasons for considering that the Bunter Sandstone of England formed dry land during the deposition of the Muschelkalk of Germany:—

1st. That the Lower Keuper Sandstone rests on an eroded surface of the Bunter; 2d, that the basement-bed of the Keuper is frequently a breccia or shingle-beach; and 3d, that there is a local unconformity observable in Stafford, Leicester, and Lancashire between these formations.

The author described the distribution of the quartzose conglomerates which form the middle division of the Bunter, and considers it probable that they are the reconstructed materials of the Old Red Conglomerate of Scotland.

The probable extension of coal-measures from the coal-fields of England to those of Belgium and France was considered, as also the bearing of the whole subject on Mr Godwin-Austen's theory of the extension of coal-measures under the chalk of the Thames Valley; and it was inferred that coal-measures might possibly be found at not unapproachable depths under parts of Oxford and Northamptonshire. It was also shown that, from indications presented by the coal-formation at the southern borders of the Staffordshire and Warwickshire coal-field, there was reason to suspect that the formation becomes attenuated and less productive of valuable coal-beds in its extension towards the south-eastern districts.

[The paper was illustrated by a series of comparative horizontal sections across the midland counties.]

May 4, 1859.—Professor J. PHILLIPS, President, in the Chair.

The following Communications were read:—

1. *On the Ossiferous Cave called "Grotta di Maccagnone," near Palermo.* By Dr H. FALCONER, F.R.S., F.G.S.

In a letter, dated Palermo, March 21, 1859, and addressed to Sir C. Lyell, V.P.G.S., Dr Falconer first states that from the Caves along the coast between Palermo and Trapani he has lately obtained remains of *Elephas antiquus, Hippopotamus Pentlandi, H. siculus, Sus priscus* (?)

Equus, *Bos*, *Cervus intermedius* and another species, *Felis*, *Ursus*, and *Canis*, and coprolites of *Hyæna;* but no remains of *Rhinoceros*, nor of *Elephas primigenius*. These additions to the previously ascertained fauna of the Cave-period in Sicily may aid in putting it in relation with the Newer Tertiary deposits of Italy.

The author then proceeds to describe the Grotta di Maccagnone, a previously undescribed ossiferous cave, in the Hippurite-limestone, westward of the Bay of Carini (between Palermo and Trapani). In the breccia below its entrance he met with remains of *Hippopotamus* in abundance, and remains of *Elephas antiquus* in the upper deposit of humus within the cave. But some other fossils were discovered under very interesting and somewhat anomalous conditions in this cave. The interior of the cavern is lined with stalagmite; and at a spot on the roof, where this is denuded, Dr Falconer found a large patch of bone-breccia containing teeth of Ruminants, bits of carbon, shells of several species of *Helix*, and a vast abundance of flint and agate knives of human manufacture. At other places, and wherever the author had the calcareous coating broken by hammers, he found similar remains. At one spot, on breaking the stalagmite, he found against the roof of the cave a thick calcareo-ochreous layer containing abundance of the coprolites of a large *Hyæna*.

Dr Falconer draws the following inferences from the study of these facts:—1. That the Maccagnone Cave was filled up to the roof within the human period, so that a thick layer of bone splinters, teeth, land-shells, and human objects was agglutinated to the roof by the infiltration of water holding lime in solution. 2. That the coprolites of a large *Hyæna* were similarly cemented to the roof at the same period. 3. That subsequently, and within the human period, such a great amount of change took place in the physical configuration of the district as to have caused the cave to be washed out and emptied of its contents, excepting the patches of material cemented to the roof and since coated with additional stalagmite.

2. *On the Jurassic Flora.* By Baron ACHILLE DE ZIGNO. Communicated by C. BUNBURY, Esq., F.G.S.

In studying the numerous specimens of Jurassic Plants discovered in the Venetian Alps, Sig. de Zigno has found it necessary to pass in revision all the known species derived from the Jurassic strata in different countries. In preparing his large work on the Fossil Plants of the Oolitic Rocks ("Flora fossilis Formationis Oolithicæ"), two parts of which have been published, the author finds, as may be expected, some discrepancies in the published opinions as to the place which the plant-bearing beds of Scania, Richmond (U. S.), India, Australia, and South Africa respectively, are entitled to in the geological scale. As the apparent weight of evidence places some of these deposits in other formations than the Jurassic, and as some are still very doubtfully placed, the author omits them from his sources of Jurassic plants.

In the two parts of his work which he has presented to the Society, the author describes the Jurassic *Calamites* (including the *Asterophyllites*), the *Phyllothecæ*, and *Equiseta*. The plates of figures accompanying the foregoing, but not yet described, are recommended by the author to the notice of English paleobotanists, as illustrative of interesting but somewhat obscure Ferns; and he particularly requests that search should be made in the Oolites of Yorkshire for specimens of *Pachypteris* with pinnules having a single midrib. Sig. de Zigno supports Sternberg and Bronn in the suggestion that under the term *Equisetites columnaris* authors have confounded two distinct forms; one from Brora and Yorkshire, with thick joints, and illustrated by König; the other being found in the Lias and Trias. Some remarks on the probable relations of *Glossopteris* and *Sagenopteris* follow.

The remains of Ferns in Jurassic-beds of the Venetian Alps are numerous, though the species are few. The fructification is often evident; and the epidermis of the fronds can be sometimes separated for microscopical examination. The *Cycadeæ* have more species; and the *Coniferæ* (especially the *Brachyphylla*) are numerous.

3. *On a Group of supposed Reptilian Eggs (Oolithes Bathonicæ) from the Great Oolite of Cirencester.* By Professor J. Buckman, F.G.S.

The specimen referred to was obtained by Mr Dalton from the Harebushes quarry, near Cirencester, and presents evidence of a compact cluster of eight oval bodies (each about 2 inches long and 1 inch across) in a mass of oolitic rock. These oval bodies being equally rounded at the ends, and in this differing from bird's eggs, the author thinks that they must have been the eggs of a reptile. The egg-shells were very thin, have been here and there puckered by pressure, and are more or less occupied with calc-spar.

[The specimen was exhibited to the meeting.]

4. *On some Sections of the Strata near Oxford.*—No. I. By Professor Phillips, Pres. G.S.

In this communication Professor Phillips gave the details of sections showing the base and the top of the Great Oolite in the Valley of the Cherwell. This oolite, with sandy layers below and variable argillaceous beds above (capped by the Cornbrash), has been entirely referred to the Great Oolite formation by the Geological Survey, and has been traced through Northamptonshire to the cuttings in the Great Northern Railway near Stamford and Grantham; and continues through Lincolnshire to the Humber. On the north of that river this series is continued by the Oolite of Brough and Cave, and is recognised again in the Millepore-rock at the base of the Gristhorpe Cliffs. Hence it appears that the calcareous shelly beds of Gristhorpe, on the Yorkshire, coast are still to be assigned, as they were in earlier works, to the Great Oolite group, notwithstanding the fact that they contain a few fossils which in the South of England are prevalent in the Inferior Oolite, together with many the distribution of which is not there limited to one member of the Great or Bath Oolite series.

May 18, 1859.—Major-Gen. Portlock, Vice-President, in the Chair.

The following Communications were read:—

1. *Palichthyologic Notes, No. 12. Remarks on the Nomenclature of the Fishes of the Old Red Sandstone.* By Sir P. Egerton, Bart., M.P., F.R.S., F.G.S., &c.

Premising with some remarks on the in many respects unsatisfactory condition of the nomenclature of the fishes of the Old Red Sandstone, the author refers to the late revival, by Dr Pander, of the discussion as to the priority of Eichwald's name "Asterolepis" over the "Pterichthys" of Agassiz; and, after a detail of the circumstances of the case, Sir Philip states that there is every reason for the retention of the name *Pterichthys* for the "winged fish" discovered at Cromarty by Miller in 1831, introduced by him to the scientific world in 1839, and named *Pterichthys* by Agassiz in 1840.

The author then proceeded to offer some critical remarks on several of the genera and species which Prof. M'Coy has described from the Old

Red Sandstone. *Chirolepis velox*, M'Coy, is regarded by him as a good species; but *C. curtus* as identical with *C. Cummingiæ*, and *C. macrocephalus* with *C. Traillii*. *Chiracanthus grandispinus* and *C. pulverulentus* are regarded as good species; but *C. lateralis* is referred to *C. minor*. *Diplacanthus gibbus* and *D. perarmatus* are accepted. The substitution of *Diplopterax* for *Diplopterus* is not considered necessary. *Diplopterus gracilis* appears to be a variety of *D. Agassizii*. The occurrence of *D. macrolepidotus* in Caithness, and the restriction of *D. macrocephalus* to Lethen-bar and Russia, are regarded as a reason for not accepting Prof. M'Coy's view as to the identity of these two forms.

Osteolepis arenatus, stated by Prof. M'Coy to occur at Orkney, has been met with only in the Gamrie by Sir Philip. *O. brevis* is regarded as a good species, though the apparent breadth of the head has probably been misunderstood. Hugh Miller has well figured and described the cranial anatomy of this species in the "Footprints." *Triplopterus Pollexfeni* is also considered to be well established, generically and specifically. Sir Philip coincides with Prof. M'Coy in classing *Dipterus* with the *Cœlacanthi*, but observes that it is distinct from *Glyptolepis*. *Dipterus* has but one anal fin. *Dipterus brachypygopterus* and *D. macropygopterus* are, in the author's opinion, synonyms; but *D. Valenciennesi* is regarded by him as distinct.

Conchodus is esteemed by the author only a provisional genus.

Sir Philip agrees with M'Coy in separating from the *Holoptychius* the large fishes of the coal-measures which have received the name *Rhizodus* from Prof. Owen. The latter have an ossified vertebral column. *Holoptychius* has decidedly two dorsal fins. Some good specimens lately obtained at Dura Den prove that *H. Andersonii* and *H. Flemingii* are specifically the same. The determination of *H. princeps* by scales alone is not regarded as satisfactory; but *H. Sedgwickii* is a good species. *Gyroptychius angustus* and *G. diplopteroides* are considered as good species of a new and important genus; but Sir Philip refers them to the *Saurodipteridæ*, not to the *Cœlacanthi*. *Platygnathus Jamiesoni*, Ag., is well founded, as proved by recent discoveries in Dura Den; but the specimen of jaw named *P. paucidens* by Agassiz is assigned to *Asterolepis* by Hugh Miller.

With regard to the *Placodermata* of M'Coy, *Pterichthys* and *Coccosteus* are the types, and *Chelyophorus* is probably a member of the family; but it is still doubtful whether *Asterolepis* and *Heterosteus* belong to it. *Cephalaspis*, *Pteraspis*, and *Auchenaspis* remain for the limited *Cephalaspidæ*.

Pterichthys had certainly one dorsal and two ventral fins.

Sir Philip remarks that in *Coccosteus* M'Coy and others have mistaken for vertebral centres the thick lower extremities of the neurapophyses; hence the *C. microspondylus* of M'Coy is a misnomer, and what he terms the "dermal bones of the dorsal fin reversed," in his specimen, are the hæmapophyses. Sir Philip thinks that *C. microspondylus* and *C. trigonaspis* must be regarded as synonyms of *C. decipiens*, Ag. *C. pusillus* is quoted as a good species, and probably the same as one subsequently described by H. Miller as *C. minor*.

In a Supplement to this Memoir, Sir P. Egerton gives several extracts from unpublished letters by the late Hugh Miller, descriptive of structural characters of the *Coccosteus*. Among these notes is the description of a small well-defined *Coccosteus* which Sir Philip proposes to signalize as *C. Milleri*. [Drawings and casts, prepared by the late Mr H. Miller, illustrated these Supplemental Notes.]

2. *On the Yellow Sandstone of Dura Den and its Fossil Fishes.* By the Rev. JOHN ANDERSON, D.D., F.G.S., &c.

In his geological remarks on Dura Den, the author described the sedimentary strata in the vicinity as consisting of (in ascending order), 1. Grey sandstone, the equivalent of the Carmylie and Forfarshire flagstones, with *Cephalaspis* and *Pterygotus*. 2. The red and mottled beds, such as those of the Carse of Gowrie, and the Clashbennie zone with *Holoptychius nobilissimus*, *Phyllolepis concentricus*, and *Glyptolepis elegans*. 3. Conglomerates, marls, and cornstone, with few and obscure fossils. 4. The Yellow Sandstone, rich in remains of *Holoptychius* and other fishes, and about 300 or 400 feet in thickness. This sandstone is seen to rest unconformably on the middle or Clashbennie series of the Old Red at the northern opening of the Den, and at the southern end is unconformably overlaid by the carboniferous rocks. It is also exposed beneath the lower coal-series of Cults, the Lomonds, Binnarty, and the Cleish Hills. It is seen also in Western Scotland (Renfrewshire and Ayrshire), and also in Berwickshire and elsewhere in the south, with its Pterichthyan and Holoptychian fossils. In the author's opinion, it is entirely distinct from the "Yellow Sandstone" of the Irish geologists.

At Dura Den the yellow sandstone in some spots teems with fossil fish, especially in one thin bed. In 1858 a remarkably fine *Holoptychius Andersoni* was met with; and this, with many other specimens, fully bears out Agassiz's conjectures for completing the form and details of the fish where his materials had been insufficient. Dr Anderson also offered some remarks on the *Glyptopomus minor* (Agass.), the specimen of which was obtained from this locality; and he drew attention to two apparently as yet undescribed fishes, also from Dura Den.

[Several specimens from Dura Den, and drawings, were exhibited by the author. And a collection of specimens from the Society's Museum, and a selection from the original drawings illustrating M. Agassiz's Monograph, were also exhibited.]

June 1, 1859.—Major-Gen. PORTLOCK, Vice-President in the Chair.

The following Communications were read:—

1. *On the Sinking for Coal at the Shireoaks Colliery, near Worksop, Notts.* By J. LANCASTER, Esq., and C. C. WRIGHT, Esq., F.G.S.

In two shafts sunk for the Duke of Newcastle on the north-west side of his estate of Worksop Manor, it was found that the Permian beds have a thickness of 166 feet,—the uppermost consisting of thin sandstone and marls (54 feet), then hard yellow limestone (54 feet), blue limestone and shale (20 feet), blue shale (33 feet), and soft gritstone, probably equivalent to the "Quicksand" of the north (5 feet). Below the gritstone the coal-measures commence with 5 feet of blue shale, in which there are four bands of ironstone; another band, 15 inches thick, lies immediately below. This iron-ore is chiefly in the state of peroxide, gives an average of 42 per cent. of metallic iron, and promises to be of great economical value. The first seam of coal (2 feet thick, and of inferior quality) was cut at a depth of 88 yards. Four yards below this is a compact sandstone 66 feet thick. The sinking through this rock occupied 20 months; each pit made 500 gallons of water a minute, which was stopped in detail by cast-iron tubing. The pressure from the gas at the bottom of this thick rock was at times as high as 210 lbs. per square inch, but is now about 196 lbs. per square inch. Shales, with coal-seams and bands of ironstone, all thin or of inferior quality, were met with in the next 170 yards.

At 346 yards the first thick coal was cut, and found to be 4 feet 6 inches thick, and of good quality. This is considered to be the "Wathwood Coal." The "Top Hard Coal" was cut at a depth of 510 yards, and found to be 3 feet 10 inches thick: the strata intervening between this and the "Wathwood Coal" were found to have much the same characters and thickness as they are known to have elsewhere. The sinkings were commenced in March 1854, and perseveringly continued until their completion on February 1st, 1859. Altogether, 37 feet of coal were passed through; but only four seams are of workable thickness. The authors of this communication remark that the district appears to be remarkably free from faults, that the dip decreases considerably towards the east, and that the "Top Hard Coal" appears to thin out eastwardly.

[This paper was illustrated by carefully prepared sections (vertical and horizontal), and by specimens of the ironstones, &c.]

2. *Notes on the Geology of Southern Australia.* By A. R. C. SELWYN, Esq., Director of the Geological Survey of Victoria. In a Letter to Sir R. I. MURCHISON, F.G.S.

Mr Selwyn remarked that, as to the impoverishment of auriferous veins in depth, the only evidence of such being the case in Victoria is the great richness of the older drifts; for, judging from the large size of the nuggets sometimes found in the gravels, compared with that of the nuggets met with in the gold-bearing quartz-veins (usually from about ½ dwt. to ½ oz., though occasionally as much as 12 oz., or even 13 lbs.), the upper portions of the veins, now ground down into gravel, were probably richer in gold (as formerly suggested) than the lower parts, now remaining. As far as actual mining experience shows, some of the "quartz-reefs" in Victoria prove as rich in gold at a depth of 200, 230, and 400 feet as at the surface; the yield, however, fluctuates at any depth yet reached. According to the author's latest observations, the gold-drifts, and their accompanying basaltic lavas, are of Pliocene and Post-pliocene age. Miocene beds occur at Corio Bay, Cape Otway coast, Murray basin, and Brighton; and Eocene beds on the east shore of Port Philip, Muddy Creek, and Hamilto.n Two silicified fossils (Echinoderm and Coral), thought by Prof. M'Coy to be of Cretaceous origin, have been found in the gravel near Melbourne.

This letter also contains some remarks on the probability of some of the coal of Eastern Victoria being of "Carboniferous" age,—on the occurrence of Silurian fossils in the rocks of all the gold-districts,—on the newly-discovered bone-cave at Gisborne, about twenty-five miles north of Melbourne,—and on the progress of the Geological Survey of the colony.

[Portions of the Geological Survey Map of Victoria, lent by the Secretary of State for the Colonies, and specimens of gold, &c., lent by Prof. Tennant, F.G.S., were exhibited in illustration of this paper.]

Fossils from Mayence, &c., presented by W. J. Hamilton, Esq., For. Sec. G.S.; Fossil Trigoniæ from South Africa, presented by Capt. Harvey, R.E.; and a series of Photolithographs of Fossil foot-tracks from Connecticut, lent by Dr Bowditch, were exhibited at this meeting.

American Scientific Association.

Superintendent Bache read a paper on *Gulf-stream Explorations.*—This memoir concerned the distribution of temperature in the Gulf-stream in the Florida Channel. It was communicated by authority of the Treasury Department. The results of the explorations of the Gulf-stream have been communicated to the Association from time to time, as facts of peculiar interest have been discovered. The original plan of these explorations was carefully studied, and having proved successful has been steadily adhered to. Recent observations have been directed to that part of the stream between Havana and Cape Florida known as the Channel and Strait of Florida. The present communication contains the results obtained on four sections in this locality, by Com. B. F. Sands and Lieut.-Com. T. A. Craven, U.S.N., assistants in the Coast Survey in 1855 and 1859. The diagrams show the geographical positions of the sections, the temperature at different depths, the depths corresponding to certain specified temperatures, and the form of the bottom, and other particulars of the sections. The sections are perpendicular to the coast at distances respectively, of about fifty, one hundred, and two hundred miles west of Cape Florida. The Strait is funnel-shaped, being about ninety miles wide at Havana, and about forty-five at Cape Florida.

The descent of the bottom of the sea from the Florida side is for the most part gradual, but from the opposite side quite abrupt, so that the greatest depth is on the Cuba side. At Havana there is an abrupt descent of nearly a mile within five miles of the shore; while on the Tortugas and Key West side, the water is comparatively shallow, and the descent gradual. This fact goes to confirm the conclusion that the stronger current of the Gulf-stream makes the circuit of the Gulf of Mexico, since, if it impinges directly on the shores of the Tortugas and the other Florida Keys, we should find its effects in the wearing of a deeper channel in the soft materials of the bottom on that side.

The law of temperature, with depth at the different positions in the stream, nearer to the shore, and beyond the stream, had been already derived from the Atlantic sections, and merely receives a confirmation in these. The cold wall is shown to exist here also, though at and near the surface the overflow of the warm water of the stream prevents the cold water from appearing at the surface.

The section from Cape Florida to Bemini shows distinctly the warm and cold bands into which the Gulf-stream is divided; and the figure of the bottom corresponds to these, the colder water following the bottom, and being raised up when it is elevated, and the reverse; the warm bands corresponding to deep water, and the colder ones to less depths. The figure of the bottom in the sections west of Cape Florida has no such elevations and depressions, the bottom having a continuous slope to the deepest part, and the stream has no divisions into warm and cold bands. The warmest water is further from the Florida side than from the opposite shore.

An examination of the longitudinal section of the Gulf-stream from the Shoal part between Cape Florida and Bemini, northward and southwestward, shows the same influence of the figure of the bottom in throwing the cold and sub-current upwards. Temperatures of 38°, 39°, and 40°, Fahrenheit, were found at depths of 600, 400, and 200 fathoms, off Tortugas, Sombrero Key, and Carysfort Lighthouse.

An account was given of direct experiments made by Mr J. M. Batchelder on the effect of pressure on Saxton's deep-sea thermometers, confirming the conclusion that at depths less than 600 fathoms the effect of pressure is insensible.

Prof. Silliman, jun., read the abstract of a paper prepared by Prof. A. P. Barnard, on the means of preventing the alteration of metallic surfaces employed to close and break a voltaic current. A peculiar substance collects gradually upon the platinum, which Prof. Barnard calls platinum black. This breaks the continuity of the current. Prof. Barnard remedies this by the use of the condenser of 1853. He finds that oiled silk or tissue-paper interposed prevents the spark.

Prof. Bache remarked that the same difficulty was found in the use of the telegraph for astronomical purposes. Iridium was tried as a remedy. The spark, indicative of a secondary current, dissipates the continuity of the current. It is therefore necessary to prevent it, and Prof. Barnard has proved that the tissue-paper will effect this.

A Vermont Whale.—A fossil whale, found in Charlotte, Vt., in August 1849, during the excavation for the Rutland and Burlington Railroad. It was found in blue clay, lying from ten to fourteen feet below the surface, the head almost four feet higher than the tail. The Irishmen who discovered it, supposing it to be the bones of a horse, wantonly broke many of them, and particularly the head. Enough of the head, however, was saved to give the blow-holes, which are at once characteristic of the whale family. Of the thirty teeth belonging to the whale, only nine were found. These, by their worn surfaces, indicate that the animal was not a young one, but an adult. Of the fifty-two vertebræ, eleven are missing, which Mr Hitchcock had endeavoured to supply by made ones, carved out of pine-wood. The caudal vertebræ are flattened horizontally, which is another important characteristic of the Whale family. The cheron bones, too, are nearly all present. The ribs are badly broken; of the twenty-six—the normal number—but five were found in a perfect state. A few of the others have been wired together and are attached to the skeleton. The sternum is very remarkable for its size and excellent preservation. It is fifteen inches in its largest diameter, and shows the indentations for the attachments of the ribs as perfectly as if it were a bone of an existing instead of a fossil whale. The anterior extremities, or fins, are also quite imperfect. The larger portion of the left fin, extending as far down as the bones of the carpus or wrist, were preserved. They well show the great strength which is so necessary in the propulsion of the animal through the water.

The length of the animal, when alive, including the intervertebral substance not here represented, was about fourteen feet.

This Vermont whale is, without doubt, of the genus Beluga, and the specific name *Vermontana*, was given by the late Prof. Z. Thomson, of Burlington, Vermont, who, with much labour and pains, saved these relics from destruction.

Geographical Distribution of Plants.—In Section B., Professor Gray read a paper on the similarity of the plants of north-eastern Asia with those of the eastern portion of North America. In many cases there is not only similarity, but even identity of species. Among instances of identity, he mentioned gentian, hoble-bush, poison-ivy, cranberry, and others, which are of the same species on both Continents, and it is probable that further researches will disclose additional instances. Even where there is a difference in species, there is generally a remarkable similarity in many of the genera on the two Continents.

It is curious also to notice that some species of plants which are indigenous to both New-England and Japan are not found on the intermediate ground of the western coast of North America and California. To account for this, it is not necessary to assume different centres of creation, from which the different Floras have spread. The present races of plants which inhabit the American Continent date much further back than the animals of the same region. A dozen or more of species of plants at present existing have been traced back at least to the post-tertiary, and very probably even to the tertiary geologic period, quite beneath the drift.

In the matter of Geographical distribution of plants, three theories are in vogue: first, that the different species have originated in the different localities in which they are found; second, that the same is true with occasional exceptions—one species having sometimes originated independently in two or three different localities; and third, that each species has originated only in a single locality, which last supposition is by far the most probable. Similarity of climate does not necessarily indicate similarity in the Floras of two countries. The vegetation of Australia is a signal illustration of this fact, being of a very peculiar type. The distribution of plants on the earth's surface is limited by natural laws, such as the interposition of large bodies of water, of high and snowy ranges of mountains, or of rainless regions, such as those of the Pacific coast. But by far the most stringent of these laws is the law of climate, as marked out by the isothermal lines. It is because the presence of the same species in this country and in the Upper Himalaya regions appears to transgress this law of climate that the fact seems so strange.

But we must remember that, since the period of the formation of the tertiary rocks, the climate of this portion of the earth has gone to two great and opposite extremes. During the Fluvial period, the temperature of the Arctic regions was so mild that the elephant, lion, buffalo and mastodon inhabited what is now the Arctic regions, and, of course, the limits of the tropical and temperate Floras must have been extended in a corresponding degree in the northern direction. Again, during the previous Glacial epoch the climate of the Arctic regions was extended southward over what is now the temperate zone, and the temperate climate existed in regions now tropical. Isothermal lines were the same then as now, and California had a climate like that of New England at the present day. The Alleghany and Cattskill mountains contain evidence of the presence during the Glacial period of plants now confined to the Arctic regions. Twice, then, have the Floras of the eastern portion of North America and the eastern portion of Asia been gradually brought together, and twice gradually separated by great climatal revolutions; and in this way a certain amount of intermingling of species has occurred.

The Laws of Storms.—Professor Loomis read a paper on the European storm of Dec. 25, 1836—a snow-storm memorable as one of the severest that ever was known in England, and which blocked up the mails for a whole night within a few miles of London—a fact that seems to have been considerably more curious there than a blockade for a week within a few miles of New York would have been considered here. From a comparison of this storm with the American storm of Dec. 20, 1836, and also the storms of Feb. 4 and 16, 1842, Professor Loomis has been led to the following generalizations: The area covered by a violent storm of rain or snow is sometimes nearly circular in form, sometimes elliptical, and sometimes very irregular. In the winter storms of the United States, the north and south diameter of the area is generally much longer than the east and west diameter.

Whenever storms are circular, the area of rain or snow is sometimes 1500 miles in diameter; when their form is elliptical, the area of rain or snow is sometimes 1000 miles wide, and 2000 or 3000 miles long.

Sometimes violent storms remain sensibly stationary for four or five days, but generally the centre of a storm has a progressive movement along the earth's surface. The rate of this progress varies from zero to 44 miles per hour. The American storms generally travel faster than the European.

Within the limits of prevalent westerly winds, when violent storms are raging, they generally advance from east to west.

Great rain and snow-storms are generally accompanied by a depression of the barometer near the centre of the storm and by a rise near its margin.

Winter storms commence gradually and generally attain their greatest violence only after a lapse of several days, and as gradually die away again. This succession of changes may occur all in one place; but oftener, though the storm may continue for a fortnight, it continues but two or three days in any one place.

For several hundred miles around a violent storm, the wind circulates around the centre in a direction contrary to the motion of the hands of a watch.

In Europe, as well as in the United States, on the north side of a great storm the prevalent winds are from the north-east, while on the south side they are from the south-west.

The force of the wind is proportioned to the magnitude and suddenness of the depression of the barometer; but very near the centre of a violent storm there is often a calm.

On the borders of the storm, near the line of maximum pressure, the wind has but little force, and tends outward from the line of greatest pressure.

The wind uniformly tends from an area of high barometer towards an area of low barometer, and this is one, probably, of the most important laws regulating the movement of the wind.

In a great storm, the centre of the area of high thermometer frequently does not coincide with that of the area of low barometer, or with the centre of the area of rain and snow.

The storms of Europe are very much modified, and sometimes in a great measure controlled, by the Alps of Switzerland. By the interposition of these mountains the air which sweeps over them is forced up to a great height, where it is suddenly cooled; its vapour is condensed; heat is accordingly liberated, by which the surrounding air is expanded, and rises above the usual limit of the atmosphere. It thence flows off laterally, leaving a diminished pressure beneath the cloud; that is, the barometer shows a diminished pressure in the neighbourhood of the mountain. The mountain thus becomes the centre of a great storm, and the storm may continue stationary for several days, being apparently held in its place by the action of the mountain.

On the Geology of the Rocky Mountain Chain in the vicinity of Santa Fé, New Mexico. By William P. Blake.—The Santa Fé Mountains are a part of the great Rocky Mountain system. From a few isolated points or knobs which scarcely appear above the general level of the broad plateau, a few miles south of Santa Fé, they gradually rise to a majestic altitude—from 10,000 to 13,000 feet—and extend northwards in a succession of lofty ranges and peaks to the head-waters of the Arkansas. The city of Santa Fé is situated at the eastern base of the range, at an altitude of nearly 7000 feet above the sea. At this point the central axis of the mountains is formed chiefly of metamorphic rocks, probably silurian and cambrian, and still older, consisting of gneissore and micaceous slates, with large numbers of intersecting granitic veins, in which a pink or red feldspar is very abundant. A ridge near Taos is not so highly metamorphosed, and the rocks are very slaty. A hornblendic slate, containing garnets, very similar to that found at Hanover, New-Hampshire, occurs here. On the western slope of the chain the metamorphic slates are overlaid by strata of the carboniferous period, and possibly by Devonian beds. The low hills and ridges directly east of the city, and at the base of the high ridges, are composed of carboniferous strata, overlaid by a deep red soil and deposits of detritus or drift from the mountains. It is possible that strata of Permian and Triassic age also occur. In an *arroyo* (ravine) one mile east of the plaza the strata are exposed, and consist of alternating beds of gray sandstone with beds of bluish-gray and reddish limestone, of varying thicknesses, and a coarse, ferruginous red sandstone at the base. The limestones are fossiliferous, containing producti, spirifers, delthyri and encrinites, characteristic of the Coal Measures. The stratification is very regular, and inclines to the west. The lower beds of sandstone show the action of varying and rapid currents at the time of their deposition, and the surfaces present fine ripple-marks of large size. A short distance south of this exposure there is a quarry in the limestone, from which blocks for the construction of the new hall for the territorial Legislature were obtained. The limestone is very hard, and full of upper carboniferous fossils.

A quarter of a mile east, the carboniferous strata are seen to rest upon the upraised edges of the metamorphic slates, at an angle of forty degrees. Here we find an alternation of strata of sandstone, shales, and limestone, with bituminous layers, and probably, in some places, seams of coal. The limestone beds are highly charged with encrinites and corals. At another point, near by, in an exposure of less extent, a similar succession of strata were found, and with them a bed of impure bituminous coal, from one to two feet thick. Beyond, in the same ravine, is a great body of bluish and black shales, with a coal-seam at the base. Several of the outcrops have been dug into, to obtain coal, but it had not been found in a seam thick enough to be worked with profit.

Anthracite Coal.—About twenty-seven miles south-west of Santa Fé, on the flanks of the Placer or Gold Mountains, there are workable beds of coal, and specimens which were procured are excellent anthracite. Several tons of it were taken out a few years since and carried to Santa Fé; but its characters, and the methods of igniting and burning it not being understood, it was not liked. The quality is excellent, and it cannot but be very valuable in that region, where timber is scarce. It is of especial importance to the country for its bearing upon the question as to the location of a railroad route to the Pacific. Here is, in all probability, an inexhaustible supply of the most appropriate fuel for locomotives. The strata with which this bed occurs are probably the prolongation of those at Santa Fé, but he had not had an opportunity to examine them closely.

On the Galisteo, about fifteen miles southwest of Santa Fé, there are extensive outcrops of ferruginous and yellow sandstones, associated with beds of black shales, which he referred to the Coal Measures. Beautiful exhibitions of diagonal stratification and ripple-marks are abundant.

Eastern Slope of the Mountains.—The formations of the eastern slope were examined at several points along the road from Santa Fé to Fort Union, and beyond to the Puerto. After passing the granitic axis the first stratified rock is a dark chocolate-coloured sandstone, dipping east, and a sandy conglomerate. A bed of limestone crops out beyond, but could not be examined. In the Great Cañon, stratified rocks on each side are well exposed to view, and are evidently in bold flexures, which die out as the distance from the mountains increases. No fossils were obtained here, but the formations at the base are believed to be carboniferous, while those above are possibly Permian and Triassic.

Beyond, to the east, the strata are nearly horizontal, and form the table-lands about the Pecos Valley. The escarpments of these table-lands, along the broad valleys of erosion, present beautiful sections of the rocks, as well as the most picturesque views. The bluffs, which rise to the height of from four to six hundred feet, exhibit a succession of white, gray, and red sandstones, with red shales and marls, and here and there a layer of snow-white gypsum.

Upper carboniferous limestone is exposed at Bernal Springs, where he obtained characteristic fossils, identical in appearance with those obtained by Mr Marcou at the Pecos villages in 1853. This is overlaid by a great thickness of reddish-gray sandstones, apparently without fossils, which may be regarded as supercarboniferous rocks—either carboniferous or Permian and Triassic, for they are conformable, and there is no line of demarcation. The colour is an unimportant characteristic, for the rock is frequently red without and white within, and parts of a bed are often white and others red, the coloration being due in part to the infiltration of ferruginous water.

Rocks at the Puerto.—Beyond Zecalote there is a second axis of granite rock similar to that at the Santa Fé range. Stratified rocks are uplifted in its eastern flank, and form a belt of low hills through which there is a remarkable pass or cut called the Puerto, or gate, leading out upon the broad prairies. There is a fine section of sandstones and shales, the latter being olive-green, and containing beds of nodular limestone. These strata, by alteration, would form red shales and bands of gypsum. They are probably the equivalents of the formations about Pecos.

Absence of Lower Carboniferous or Mountain Limestone.—In all these sections he failed to recognise any well-defined bed of limestone at the base of the Coal Measures containing fossils characteristic of the subcarboniferous limestone. It appears to be absent at the localities visited, the Coal Measures resting directly upon the upturned edges of the older rocks, though it is possible that the thickly-bedded red sandstone will be found to be the equivalent of the Devonian.

It is certainly very interesting to find beds of coal so far west characterized by fossils identical with those of the Appalachian coal-field, and at an elevation of from 7000 to 12,000 feet above the sea; for it is probable that in places the coal extends to the very summit of the mountain with the associated rocks, which are known so to occur.

The beds of limestone, so far as seen, are thin, not exceeding forty feet in any one bed. Mr Blake could not venture to assign the thickness of the whole series of the rocks which must be referred to the Coal Measures there, but it was probably less than 1000 feet.

Cretaceous.—No fossils or other indications of Jurassic rocks were obscured. Cretaceous strata were not seen along the Santa Fé road until the Puerto was passed beyond Fort Union. This formation presents bold bluffs along the north fork of the Canadian, with beds of whity limestone containing Inoceramui.

Volcanic Rocks.—The table-lands of the Rio Grande, especially those on the west side, at the base of the Sierra Madre, are generally capped by horizontal layers of basaltic lava, forming mural faces along the streams and cañons. These are well described by Messrs Peck and Abert. On the eastern slope of the mountains, broad lava plains are seen in the neighbourhood of Fort Union, and it is found far out upon the prairies in the isolated mounds known as Rabbit Ear and Wagon Mound. The former has the appearance of a volcanic cone, now extinct, but with the crater well defined. It was in all probability a post-cretaceous volcano.

Mineral Resources—Gold Mines.—The mineral resources of the region are extensive and varied, coal, iron, copper, lead, gold and silver, being found in quantity; but having already made a communication on this subject to the Boston Society of Natural History, no further reference was deemed necessary. He would, however, again call attention to the probability that the gold placers of New Mexico, so long known and worked with success, are probably connected with the recently discovered mines at Pike's Peak and on the head waters of the Arkansas.

The examination of the carboniferous strata of the Rocky Mountains led him to conclude that the wide expanse of the great carboniferous sea which covered the continent was broken at intervals by islands along the course of the Rocky Mountain chain, and that we there had shores from which in part the materials for the coarse-grained thickly-bedded sandstones were derived, and that, as in the Appalachians, the sandstones of the Coal Measures predominate in bulk over the limestones, which last may be believed to gradually increase in thickness towards the Mississippi and southerly, while the sandstone beds thin out. Thus while we find the Coal Measures thinning out from a thickness of six to ten thousand feet in Pennsylvania to about seven hundred or a thousand feet in Missouri, we will probably find a point of maximum thinness somewhere in Eastern Kansas, and a gradual increase of thickness of the sandstones, at least westward toward the line of the Rocky Mountains. He referred the present great elevation of the strata, 12,000 feet above the sea at some points, not only to a great continental elevation, but to local uplifts and plications. This last is an interesting point—the existence in the distant and central chain of the Rocky Mountains of a geological structure corresponding to the plications and flexures of the Appallachians.

In regard to the existence of Permian strata and those of the secondary period, he wished to be fully understood. The only horizons he had identified by fossils were the Coal Measures and the cretaceous. Between these, in that region, there is a series of strata in which we have in all probability representatives of the Permian, Triassic, and Jurassic formations—a probability rendered strong by the recent discoveries of Permian strata by Dr Shumard in the Guadalupe mountains, much farther south in the Rocky Mountain system, and in Kansas by Messrs Meek and Hayden, who have also brought Jurassic fossils from the Black Hills in the north.

SCIENTIFIC INTELLIGENCE.

CHEMISTRY.

Meteoric Iron.—M. Hugo Müller gives the following three analyses of the meteoric iron of Zacatecas, in Mexico :—

	I.	II.	III.
Iron, ...	89·84	91·30	90·91
Nickel, ...	5,96	5·82	5·65
Cobalt, ...	0·62	0·41	0·42
Phosphorus, . . .	...	0·25	0·23
Sulphur, ...	0·13	...	0·07
Silica, ...	...	...	0·50
Copper, ...	trace.	trace.	trace.
Magnesia, ...	trace.	trace.	trace.
Insoluble residuum, .	3·08	2·19	2·72
	99·63	99·97	100·50

BOTANY.

Vascular Bundles of Ferns.—It is generally stated that the vessels found in these bundles are scalariform and pitted vessels. This may be true in regard to the full-grown stem of tree ferns, but it is not so in regard to the petioles and the ribs of the young fronds of other fibres. M. Paul Biot says, that if a vertical section is made of the young circinate frond of Polypodium, Adiantum, Pteris, Asplenium ,and Dicksonia, there will be seen all kinds of vessels, and among them true unrollable spirals. The extremity of the petiole may be broken in such a way as to have a fragment supported by means of spiral threads, just as happens in the young stem of the vine or the elder. In *Polypodium vulgare* and *Lastrea Filix-mas*, these spiral vessels or tracheæ appear the only ones found at the summit of the frond during its early growth. Soon, however, their absolute and relative number diminishes, and annulated, reticulated, and scalariform vessels appear. In the early period of the development, the scalariform vessels are very rare. Their number augments as the tissues become more dense. In the old and fully developed fern-stems, scalariform vessels are almost the only ones found. Even in them, however, we meet with mixed vessels of a spiral and annular kind.—*Proceedings of Philomathic Society of Paris, July* 1859.

Respiration of Plants.—M. Traube has arrived at the following conclusions on the subject:—

1. Plants absorb oxygen, not only during germination, but during all tho periods of their growth, and even in sunlight.—(*Saussure*).
2. The absorption of oxygen is absolutely necessary for their development. If they are deprived of this gas, they cease to grow, and soon die
3. The oxygen which plants absorb in darkness is always converted into carbonic acid. This phenomenon also takes place during the day; but the presence of the acid is then detected with difficulty, owing to its decomposition by the green parts of plants.
4. Plants, besides this power of decomposing carbonic acid by means of their green parts, possess a respiration like that of animals. This respiration consists in the absorption of oxygen and the giving out of carbonic acid. It is necessary for the vital activity of their organism.
5. Plants do not possess special organs of respiration.
6. The most important product of plant-respiration is cellulose, which arises from the oxidation of a hydrated carburet, dextrine, glucose, &c.
7. The principal functions of respiration in plants is the organization and elaboration of the nourishing sap—an elaboration which depends on the presence of cellulose. The formation of cellulose is completely independent of solar light. Plants, like animals, are developed also in darkness.
8. The vertical direction seen in the development of the young plants has also no connection with sunlight.—*Trans. Acad. Sc., Berlin.* 1859.

Botany of the Rocky Mountains.—The following plants were gathered by M. Bourgeau, close to perpetual snow:—*Silene acaulis*, *Arnica*, *Menziesia ? Pedicularis*, *Gnaphalium*, *Erigeron*, *Artemisia*, *Saussurea*, *Luzula*, *Saxifraga*, *Draba*, *Androsace*, *Vaccinium*, *Salix herbacea*, *Poa alpina*, *Aspidium*, *Valeriana*, *Aquilegia*, *Dryas octopetala*, *Epilobium*. The nearest tree to the snow is *Abies alba*, which assumes the appearance of the common juniper, with which it grows, trailing along the ground. The Alpine region is from 6500 to 8000 feet in elevation. The vegetation is not rich in species. The mountains are barren, with few streams, and little humidity, and no pastures like those of the Alps. In the Rocky Mountains, streams are scarce in the southern slopes; on the northern, water is abundant, owing to the snow; but they are only little torrents, sunk deep in the rock. The plants in the forest are for the most part common in the woods of the Saskatchewan plains. The number of species is nearly in the same proportion on the mountains as in the other parts of the country. They are few in number, but each species is abundant; and each mountain, at the same elevation, bears the same species, both on the north and on the south.—*Lin. Soc. Proc.* 1859.

Felling of Forests causing Dryness of Climate.—Fontenay and Provence are places where this has taken place, according to Professor Laurent of Nancy. Wells and pits have become dry on this account. In the whole of the eastern Pyrenees and the Herault, the felling of timber has been attended with serious consequences. The temperature became higher, wells and water-courses diminished, and the dryness of the climate was much increased. Many Eastern nations have suffered from the felling of timber; for instance, Babylon, Nineveh, Thebes, Memphis, Carthage, Palestine, and the Troad. In the Vosges, injury has been caused by the destruction of forests; also in the department of Gard, at Nismes, at Bezieres, and Isere. By the destruction of forests in France, in order to replace them by cultivated fields, the temperature has become very irregular. Heavy rains, storms, and dryness, have each done their work upon the soil, and crops have been every year more and more uncertain.—*Laurent, de l'Influence de la Culture sur l'Atmosphere*, &c.

Vegetable Parasites in the Hard Structures of Animals.—Quekett showed in his lectures on Histology, that vegetable parasites, as confervæ, occur frequently in the skeletons of coral. Rose and Clarapede also showed tubular structures of a similar nature, as occurring on fossil fish scales, and on shells. Wedl and Kölliker have lately examined the subject, and have found these parasites on many hard animal structures. Wedl's observations concern only parasites in the shells of bivalves and gasteropods. Kölliker has found what he calls unicellular fungi, with sporangia, in sponges, foraminifera, corals, bivalves, brachiopods, gasteropods, annelids, cirrhipeds, and fishes.

Kölliker thinks it possible that the parasites dissolve the carbonate of lime of the hard structures into which they penetrate by means of exudation of carbonic acid, which secretion would seem to take place only at the growing ends of the spongial tubes, as they never lie in large cavities, but are always closely surrounded by the calcareous mass. In some cases, as in the horny fibres of sponges, it seems probable that the parasites simply bore these canals by mechanical force, as is the case when vegetable parasites make their way through the cell-membranes of confervæ and other plants. Besides this, it deserves also to be remembered, that nearly all the parasites spoken of occur in marine animals.—*Proceedings of the Royal Society of London.* 1859.

Ægilops triticoides.—This plant, which was considered by M. Fabre as a stage in the transition of *Ægilops ovata* into cultivated wheat, has been shown by Godron to be a hybrid procured from *Æ. ovata*, fertilized by the pollen of Wheat. Regel, in Germany, Vilmorin and Groenland in Paris, and Planchon at Montpellier, have confirmed this statement. *Ægilops triticoides* is generally sterile, but it sometimes bears fertile seeds. These seeds, when sown, have produced plants called by M. Fabre *Ægilops speltiformis*. This has been shown by Godron to be a hybrid between *Ægilops triticoides* and *Triticum vulgare* (common wheat).—*Comptes Rendus*, 1858.

On the Coiling of Tendrils. By Dr ASA GRAY.—Tendrils, in several common plants, will coil up more or less, principally after being touched or brought with a slight force into contact with a foreign body. In some plants, the movement of coiling is rapid enough to be directly seen by the eye. The tendrils of some cucurbitaceæ,—as *Sieyos angulatus*, the burr cucumber,—after coiling, in consequence of touch, will uncoil into a straight position in the course of an hour, and will again coil up at a second touch. This may be repeated three or four times in the course of six or seven hours. A certain temperature seems to be necessary. Gray experimented at 77° F. A tendril which was straight, except a slight hook at the top, on being gently touched once or twice with a piece of wood on the upper side, coiled at the end into two and a half to three turns within a minute and a half. The motion began after an interval of several seconds, and fully half of the coiling was quick enough to be very distinctly seen. After a little more than an hour had elapsed it was found to be straight again. The contact was repeated. The coiling began within four seconds, and made one circle and a quarter in about four seconds. It had straightened again in one hour and five minutes; and it coiled the third time on being touched rather firmly, but not so quickly as before, viz., one and a half times in half a minute. The same movements have been observed in the tendrils of the grape vine. The coiling is perhaps caused by contraction of cells on concave side of coil.—*Asa Gray in Proceedings of American Academy of Science and Art.*

ZOOLOGY.

On Spontaneous Generation. By MILNE EDWARDS.—Physiologists have long been devided on the subject of the origin of life in organized beings. The larger part believe that this force exists only where it has been transmitted; that from the creation of the species till the present time, an uninterrupted chain of possessors of this power has communicated it successively; and that dead matter has no power of organizing a plant or an animal unless it be submitted to the action of a living being or a germ that has proceeded from an individual of some species.

Others, on the contrary, have held that inert matter, under certain chemical and physical conditions, could take on life without the agency of a generating being; that plants and animals may produce themselves in all their parts without deriving the principle of existence from another living body; and that consequently life itself must be considered, not as a force which has been imparted peculiarly to organized beings, but as a general property of organizable matter manifesting itself under certain favourable conditions.

In my lectures and writings, I have often combated this last doctrine; and the hypothesis of *spontaneous generation* has to-day so few supporters among zoologists, that I should have feared to abuse the patience of the Academy in discussing it at this time, had I not seen in the Report of a recent session of this body, that one of our correspondents, Mr Pouchet, had made it the object of new researches, and had arrived at conclusions which, if right, sustain the idea that living beings may be made by the same general forces on which chemical combinations in inorganic nature depend. Since reading this memoir, I have thought it might be useful to submit to the judgment of my colleagues my reasons for rejecting its conclusions; and it appears to me desirable also to know the opinions of other physiologists on a point of so much importance: besides, the question reaches beyond the domain of the natural sciences, and we may look for additional light from our chemists.

Long before the invention of the microscope had enabled zoologists to discover the animalcules which are produced in myriads in waters containing an infusion of organic matters, it has been observed that dead bodies, when left to putrefy, often became populated with swarms of life; and as the intervention of no living being was manifest in their production, the old naturalists supposed them a product of the putrefaction which was in progress, believing that the material, after ceasing to pertain to a living being, could reorganise itself under a new form, and so constitute animals which had no parent; accordingly, that life is not the cause, but the consequence, of a certain mode of arrangement of the molecules composing these substances, and that this kind of molecular grouping could be determined by inorganic forces in nature.

The occurrence of maggots in carrion was one of the cases. But since the study of the origin of these animals by the Florentine Academy, happily named "del Cimento," and the exact investigations of Redi, one of its members, it has been well understood that these worms about dead bodies, far from being a result of spontaneous generation, are the brood of well-known insects, species which find in such bodies the conditions requisite for development, and hence, through a marvellous instinct, deposit there their eggs.

The experiments of Redi, which date from the middle of the seventeenth century, left no uncertainty respecting these larvæ. But while very easy to establish the fact respecting animals as large as flies, it was far less so with regard to infusory animalcules, which are discernible only by means of the microscope, and whose germs are so excessively minute that they have escaped all the methods of observation which the science of optics has supplied. When, therefore, Lewenhoek and his successors made known the existence of these animalcules, the hypothesis of spontaneous generation regained favour. While some physiologists regarded them as derived from germs of extreme minuteness which were spread everywhere in nature, and floating as fine dust in the atmosphere, settled on all bodies to develop only where the conditions of air, water, and organic decomposition favoured; others denied the existence of germs, and supposed that under the dissolving action of the water the dead organic substance took on life, and so came out as new beings.

Analogy afforded a strong argument for the first of these opinions. The second has often been sustained by appeals to researches claiming that animalcules were produced under circumstances in which all germs from external sources were excluded, and all present in the waters used had been destroyed. Frey and several other observers have thought that they had succeeded in securing these conditions, and still had found their infusions populated with microscopic plants and animals; whence the conclusion that these organisms were a result of spontaneous generation.

It does not pertain to me to pronounce on the origin of microscopic plants, for this difficult subject must be left to botanists. But as regards animals, I do not hesitate to say that the experimental conditions required to prove the truth of spontaneous generation have not been realized by any of the predecessors of Mr Pouchet. And are the researches of this naturalist, that have recently been communicated to the Academy, free from the objections which are made against earlier experiments? I believe not: and before mentioning some observations I have had occasion to make on this subject, I will briefly state the reasons that lead me to this conclusion.

I do not question the facts stated by Mr Pouchet. The point is, have these facts the significance attributed to them? I believe not. His experiment is briefly as follows. After having boiled some water, and kept the liquid from contact with the air, he puts it into contact with pure oxygen, and introduces a certain quantity of hay, which had been previously enclosed in a flask and heated for a half-hour in a stove, whose heat was carried up to 100° C., or to the boiling-point of water. The infusion thus prepared was hermetically sealed, and after some days Mr Pouchet found infusoria developed in it.

To make these facts sure proof that the animalcules obtained were not derived from the hay put into the infusion, it must be shown that the heat of the stove had destroyed all the germs. Mr Pouchet presumes that this is true, because on boiling in water the spores of a *Penecillum*, he has seen that they were decomposed. But this reason does not satisfy me.

In the first place, was the hay, although enclosed in a flask and kept thirty minutes in a stove at 100° C. (212° F.), really carried up to the temperature of boiling water? Mr Pouchet believes it; but I think to the contrary, and I think that physicists and chemists will judge so too. The equilibrium of temperature under such conditions is not established so promptly as this; it appears to me probable that the hay, enclosed in a glass vessel, and surrounded by air in repose,—both substances bad conductors of heat,—was in reality heated but little by the heat of the stove during the short time it was exposed to it.

But supposing that the hay was heated up to 100° C., can we then conclude that the germs had lost their vitality and were incapable of development? No; for there is an important distinction here to be recognised between the action of heat on organized bodies which contain water, and on those which are in the dry state. This follows directly from the researches, already old, of our learned colleague, Mr Chevreul. Although, in ordinary circumstances, death takes place when animals are exposed to a temperature sufficient to determine the coagulation of the hydrated albumen in their tissues, we know that this is not always so in the case of those which have been previously dried. In fact, fifteen years since, Mr Doyère made known that certain animalcules, such as the *Tardigrades*,* after being sufficiently dried, would preserve their vitality for several hours while exposed in a stove whose temperature is much higher than that used by Mr Pouchet for his flask of hay. I have seen these animalcules resist thus the very prolonged action of a stove whose temperature stood at 120° C. (248° F.); and in the researches of Mr Doyère, the heat of the ambient medium was carried to 140° C. (284° F.), without death ensuing from the heat.

What is true for the *Tardigrades*, animals of a very complex structure, may also be true for the germs of Infusoria in general; and I conclude that nothing in the trials of Mr Pouchet authorizes us to infer that the germs of the animalcules obtained by this naturalist were not in the hay that was used in his experiment. I will even say that the experiments of our correspondent do not seem to me to add any new probability in favour of the hypothesis of spontaneous generation.

I have often made analogous experiments; and I have always found that the living animalcules which appeared in water containing dead organic matters, were increasingly rare the more complete the precautions employed for protecting the liquids from the introduction of germs. In more than one trial, I should have believed that spontaneous generation had taken place under my own eye, had I not, on reflecting on the conditions under which I was operating, perceived sources of error, and on setting these aside observed negative results to multiply.

I will not occupy the Academy with the general recital of these trials, but will ask permission to recount briefly a single series of experiments in which some infusions, that if exposed to the air would in all probability have given birth to animalcules, afforded none when the imprisoned matters in the hermetically-sealed vessel had been subjected to a temperature high enough to cause the coagulation of the contained albuminoid substances.

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* The Tardigrade animalcules are minute worm-shaped animals, about a fortieth of an inch in length, belonging to the Rotatoria of Ehrenberg, and therefore much higher in structure than the ordinary Infusoria.

I placed in two tubes, having the form of test-tubes, the water and the organic matters for the trial. One of these tubes, which was two-thirds filled with air, was then closed by means of a lamp, and both this and the other tube were then plunged into a bath of boiling water. The bath was kept in ebullition long enough to establish an equilibrium between the water outside and the liquid of the two infusions; and then the tubes were allowed to cool and left to themselves, care being taken to examine the contents from time to time. After some days, I found animalcules in the tube which remained open to the atmosphere, but *not a single one in that which had been hermetically sealed.*

I have been accustomed to cite these experiments in my lectures, but had not thought of bringing them before the Academy, because negative results acquire importance only when they have been obtained constantly in a large number of trials, and also because the spontaneous generation of animals appears to me so little probable that I would not devote time to the repetition of researches on a subject which seems to be already settled. Only in view of the communication of our correspondent, and the interest that experimenting in this direction may excite in our young physiologists, have I been induced to bring out these facts among the reasons for still rejecting the hypothesis of spontaneous generation as an explanation of facts connected with the multiplication of animalcules.

An hypothesis which is not necessary in order to understand the phenomena made known by observations, and which is in flagrant discordance with all that analogy teaches us, seems to have no right to a place in science. It may be that chemistry will be able to make all the kinds of substances which occur in the constitution of living bodies; but as to the genesis of living organisms without the concurrence of vital force, I see no reason for believing it. Until more amply instructed, I shall therefore continue to think that in the animal kingdom there is no such thing as spontaneous generation, and that all animals, large and small, are subject to the same law, and can exist only when they have been generated by human beings.—*Comptes Rendus*, 1859.

Note on Spontaneous Generation, by JAMES D. DANA.—1. There is a well-known principle in the system of nature that deserves to be considered in this connection. The principle is so fully sustained by all research both in chemistry and zoology, including the important experiments above mentioned, that it may well carry with it great weight, and quiet both apprehension and expectation on this subject. It is this:—The forces in life and inorganic nature act in opposite directions—the former *upward*, the latter *downward*.

The vital force, in the organic substances it forms, *ascends* through vegetable and animal life to an exalted height in the scale of compounds at an extreme remove from saturation with oxygen; inorganic force *descends* towards the saturated oxide. The former reaches a point which from its very elevation is one of great *instability*; the latter tends towards one of perfect *stability*. There is hence a counterpart, or cyclical relation, between the two great lines of action in nature.

As some readers of these remarks may not be familiar with chemistry, a further word of explanation is added.

When an element unites with its full allowance of oxygen, as determined by its affinities, it is in a sense saturated with it. Since the attraction of the elements for oxygen is the most universal, and, in general, the strongest in nature, the oxides, as a class, are the most stable of compounds; the rocks, the earth's foundations, are made of them. But evanescence and unceasing change are in the fundamental idea of the living structure; and consequently the material of the plant or animal contains only oxygen enough to give increased instability to the combination. Moreover the compounds augment in instability, through this and other ways, with the rise in the grade of organic life, and reach, probably, their farthest extreme in this respect in the brain. Here, then, is the summit of the series of compounds which arise under the agency of life. The stable oxide is at the lower end of the series in nature, the material of the brain at the upper. Passing from the latter condition towards the former is therefore a real descent; and it is the natural downward course of inorganic forces;—while passing towards the latter is as truly an ascent; it is the counter-movement of life.

The plant through its vital functions may take carbonic acid, and from it continue to elaborate the organic products constituting vegetable fibre, until a whole tree of such material is made, and then produce the higher material of the flower and seed. The animal may then go to the plants and use them in making a still higher class of products, muscular fibre and nerve. After all this is done, now turn over the material to the action of chemical and physical forces,—and the work of years of life is soon pulled down from its height, and one part after another descends towards that state of comparative inactivity, the condition of an oxide. Chemistry makes organic products by commencing with those of a higher grade than the kind to be made, but not otherwise. Albumen is a prominent material of the egg; and chemistry has not succeeded in making dead albumen, much less living.

The very relation of life to chemistry is therefore evidence that chemistry cannot make life; it works in just the reverse direction. And in this reciprocal relation one of the profoundest laws of nature is exhibited. It leads the mind to recognise one author for both, and not to imagine that one side in the cycle has generated the other.

2. There is another consideration, which, if it has not the force of demonstration, may help the mind to understand the extent of the transition from dead matter to living.

(*a*) In ordinary *inorganic* composition, there is the simple formation of inorganic particles, and, on consolidation, their aggregation into crystals, the perfect individuals of inorganic nature. With the enlargement of the crystal there is no gain of new powers or qualities: it simply exists. In fact, in entering this state of perfection, there is *a loss of latent force;* for the gas is the highest condition of store or magazined force in inorganic nature, the liquid the next, and the solid the lowest, this condition of power being related directly to the amount of heat.

(*b*) The *plant* grows from its germ, enlarges, accumulates force, storing it away in vegetable fibre, and accomplishes its highest functions in its blossoms and fruit. But there is here only *latent or stored force* generated, besides that which is used up in growth, and *no mechanical* force. The minute spore or reproductive cellule of some seaweeds has locomotive power, but it is lost at the commencement of germination; and the plant is ever after as incapable of self-locomotion as a rock.

(*c*) In the *animal*, there is not only a storing of force in animal products (the fifth and highest grade of stored force in nature), but there is also increasing *mechanical* force from the first beginning of development. It is almost, or quite zero in the germ; but from this, it goes on increasing, until, in the horse, it gets to be a one-horse power; or in the ant, a one-ant power; and so for each species. And in addition to mechanical force, there is, in the higher group, the more exalted *mental force;* for the mind, while not itself material, is yet so dependent on the material, that its actions draws deeply upon the energies of the body. To make an animal germ is then to make a particle of albuminoid substance that will grow and spontaneously develop a powerful piece of enginery, and continue a system of such generations through ages of reproduction.

The creation of any such animal germ out of dead carbon, nitrogen, hydrogen, and oxygen, or any of their dead compounds, is therefore opposed to all known action or law of chemical forces; and as much so, the creation of a vegetable germ from inorganic elements.

Moreover, it is seen that the two kingdoms, the vegetable and animal, have their specific limits and comprehensive reciprocal relations, and are obviously embraced as parts of one idea in a single primal plan:—not a plan involving the generation of one out of the other, or of either out of inorganic nature, but of the three, through some Creating Power higher than all.

MISCELLANEOUS.

Japan.—In a commercial point of view, the field of Japan is very promising. We know that in former days, three centuries ago, Japanese vessels traded as far as Bengal, and that it was only the certainty of being put to death by the famous edict against foreign trading that put it down in 1637. We know that the Portuguese annually exported from Nagasaki, in the time of free intercourse, the enormous amount of 300 tons of gold annually! and that in the year 1636, four of their ships carried to Macao no less than 2,300,000 taels alone. We know at this present hour that a gold kebang, equal in real value to a British sovereign, may be bought at Nagasaki for an ounce of silver, or little more than the Mexican dollar. We know that a quantity of silk or crape, which could not be purchased at Shanghai for twenty dollars, may be had at Nagasaki for very much less. We know that the climate of Japan will not admit of the growth of tropical produce, and that the severity of its winter must occasion wants which other parts of the world can supply. Here, then, are the elements of a future commerce, and the intelligence, energy, and wealth of its rulers and people will assuredly do the rest. Silk, copper, gold, tea, and paper, apart from articles of manufacture, such as porcelain, bronzes, lacquer-ware, &c., in which Japan excels, will be at first, we should opine, their principal exports. Rice they have in profusion, and of excellent quality; the short distance of Japan from Shanghai may, in times of scarcity in Northern China, render it a valuable article of commerce. Wood, coal, and iron, are abundant; the two former obtainable at almost nominal prices. Without being learned in the mysteries of the silk trade, we cannot help thinking that its abundance in Japan must next year affect our European markets. The Japanese tea is of a fine, full flavour, well adapted to the tastes of all classes in Great Britain. The Japanese themselves prefer their own good teas to those of China, and we agree with them. Copper must be very plentiful; it has yielded enormous profits to the Dutchmen during the centuries they have had the monopoly of the trade, yet it is seen everywhere, and in everything. The brass guns alone, mounted at Nagasaki and Yeddo, would pay the ransom of a nation; the piles of their bridges are protected with sheets of it; the bottoms of their native vessels, the gunwales and stems of their boats, the stirrups of their saddles, the roofs of their temples, hilts of their swords,—in short, almost everything you see or touch has brass or copper about it, in some shape or other, and generally in profusion. Gold, for some reason or other, you never see; tradition says it is because the excessive cupidity of European nations alarmed the Japanese rulers, and that they were, and are, still anxious to keep hidden the great stores of that valuable mineral which Japan must contain, if the Dutch writer, Kæmpfer, told the truth—and there is every reason to believe he did—about the amount of the Portuguese exports in 1636.—*North China Herald.*

Transformation of Woody Fibre into Sugar.—On the occasion of the above discussion Pelouze announced the important results which follow. Cellulose precipitated from its solution in ammoniacal oxide of copper by a feeble acid is soluble in dilute chlorohydric acid. Ordinary cellulose is soluble in concentrated chlorohydric acid; water forms with this solution a precipitate of dazzling whiteness; at the end of two days the precipitate ceases to form, and all the cellulose has been transformed into sugar affording the characteristics of glucose.

The transformation of cellulose into glucose can be effected by a prolonged ebullition in water containing a small quantity of sulphuric or chlorohydric acid (some hundredths); paper, old linen, sawdust, and any cellulose more or less pure, can be thus turned into sugar at the end of several hours' boiling.

Pelouze thinks that this reaction will become the basis of a new branch of industry—one which has often been attempted since Braconnot succeeded in 1819 in transforming lignine into glucose; he thinks that the transformation would be rendered much more active by operating in a close vessel at an elevated temperature.

Lastly, Pelouze announces that, by treating cellulose with caustic potassa in fusion at a temperature between 150° and 190° C., and dissolving the product in water, a substance can be separated from it by acids which has the composition of cellulose, but differs from it in that it is soluble in the cold in alkalies; it changes into sugar in the presence of chlorohydric, acid.—*Silliman's American Journal of Science and Arts for July* 1859.

Manufacture of Aluminium.—This manufacture, which is becoming more and more extended, has just taken two steps onward; one through the publication by H. St Claire Deville, of a treatise expressly on the subject; the other, by the discovery of a process of soldering. All the labours expended on aluminium up to the month of March 1859 are recounted by Deville, and as the author and founder of this manufacture we can feel very certain that the work is not a simple compilation.

As respects the soldering of this metal, until very lately quite imperfect results have been attained. In the Universal Exhibition of 1855, there were pieces of aluminium soldered with zinc or with tin, but this weak solder did not give any solidity. Others have tried to solder with alloys of zinc, silver and aluminium. Mr Denis of Nancy has noticed that whenever aluminium and the solder melted over its surface was touched with a slip of zinc, the adhesion took place with great rapidity, as if a peculiar electric action gave it an impulse at the moment of contact; but this solder also has failed to afford much strength.

At last it has been suggested that the difficulty might be surmounted by previously coating the piece with copper, and then soldering together the coppered surfaces. In order to effect this, the aluminium, or at least the parts to be soldered, are plunged into a bath acid of sulphate of copper. The positive pole of the battery is put in direct communication with the bath, and the pieces to be coppered are touched with the negative pole; the deposit of copper takes place very regularly over the surface of the aluminium. These surfaces, thus prepared, are soldered in the ordinary way.

All these processes are, as is seen, very imperfect, and they now have only a historical interest, on account of a new and perfect method of soldering just discovered. The inventor is a gilder and silverer of metals, belonging to Paris, named Mourey; he has recently announced his process in a public meeting of the *Société d'Encouragement.* The alloy employed is composed of zinc and aluminium; Mr Mourey employs five different varieties of it according to the article to be soldered; the composition is as follows:—

	I.	II.	III.	IV.	V.
Zinc,	**80**	**85**	**88**	**92**	**94**
Aluminium,	**20**	**15**	**12**	**8**	**6**

To prepare it, he melts the aluminium in a crucible of graphite, the metal having been reduced to fragments and added little by little; when the mass is in fusion it is stirred with an iron rod while the zinc is added in small quantities at a time; the alloy is still stirred while a little tallow is added to prevent the oxydation of the zinc, and then it is cast in small ingots. It is important to avoid too high a temperature, lest the zinc should be volatized. It is also important that the zinc should be free from iron.

These five alloys have different points of fusion. Alloy No. 1 is the hardest, the others are softer in regular succession.

As for the manipulation of the solder, this comes under technology: Mr Mourey has described it in detail; but it would be going too much into specialities for us to cite his account of it, and we subjoin only a few facts interesting in a scientific point of view.

The instrument which is used in the soldering, and which is called in French "*fer-a-souder*," ought not in soldering aluminium to be either of iron or copper, but of aluminium itself; for the soldering alloy adheres to iron or copper in preference to aluminium. The flux used to facilitate the adhesion is made of three parts balsam of copaiba, mixed with one part of pure turpentine; the materials are mixed in a porcelain capsule, and a few drops of lemon-juice are added to favour the mixture of the two resins.

This flux is used for thoroughly impregnating the fragments of solder which are to be employed. It is important to use the blowpipe no longer than is necessary, to prevent loss of zinc from volatilization.

Lastly, another novelty of this branch of manufacture is aluminium bronze, which has the proportion of ten parts of aluminium to ninety of copper, and has the tenacity of steel. This alloy is now applied on a large scale by J. M. Christoffle; he has noticed that it is of great advantage to make all the surfaces of friction in machinery of aluminium-bronze. Thus a bearing which had been placed on a polishing-lathe making 2200 revolutions a minute was found to last eighteen months, while bearings of other different metal had, in the same circumstances, lasted at most only three months. He has employed this bronze with equal success in the manufacture of cannon, howitzers, and all kinds of weapons of war. Pistol-barrels have been thus made which have done good service.

There is as yet nothing very conclusive with regard to this application to artillery; but Mr Christoffle, relying on the tenacity of aluminium-bronze and its resistance to wear, thinks that it will be applicable to the manufacture of bronze for cannons. As in France large artillery-pieces are constructed exclusively in the government workshops, he has asked for a permit to manufacture at his own expense some pieces of artillery, especially such as are most exposed to injury.—*Silliman's American Journal of Science and Arts for July* 1859.

Notes on North American Crustacea, No. I. By WILLIAM STIMPSON. 48 pp. 8vo, with 1 plate (from the Annals of the Lyceum of Natural History of New York for March 1858).—We have barely space to announce the appearance of this first part of a systematic account of North American Crustacea. It commences with the Maioids and closes with the Paguras family among the Anomoura.—*Silliman's American Journal of Science and Arts for July* 1859.

BIOGRAPHY.

Biographical Sketch of David Skene, M.D., of Aberdeen. By ALEXANDER THOMSON, Esq. of Banchory.*

The purpose of this paper is to preserve and arrange what memorials can now be recovered of one who, during a short life, did much for natural science in Scotland, and whose memory has been allowed nearly to perish.

David Skene was born 13th August 1731. His grandfather, Andrew Skene, and his father, also Andrew Skene, were both eminent physicians in Aberdeen.

From manuscripts still existing in every branch of natural history, which are probably but a part of what he wrote, it appears that Skene pursued the study of nature to an extent and with an accuracy previously unknown in Scotland; and from letters addressed to him by some of the most eminent men of the time, it is evident that his merits were thoroughly recognised by his contemporaries. His early death prevented his giving any part of the fruit of his labours to the public, but it appears that he was gradually preparing several of his manuscripts for ultimate publication, and it is impossible to say how much science in Scotland may have been indebted to his personal exertions and to the stimulus to inquiry which he gave to all with whom he associated or corresponded.

His early education was conducted in Aberdeen under his father's care. It is uncertain whether he studied at King's or Marischal College, for his name does not occur in the matriculation-books of either university. In 1751 he went to Edinburgh to carry forward his professional studies, and was introduced by his father to most of the medical professors.

From his letters we find that he studied anatomy with Munro, and practice of physic and clinical lectures with Rutherford, besides attending the Infirmary. He devoted his time rigidly to his professional studies, and, in his anxiety to make progress in them, denied himself the pleasure of attending other classes to which his tastes would have led him. He worked hard in reading at home, and writing out notes of what he heard in the class-rooms as well as of what he saw in the Infirmary. At the commencement of the session, while the classes were free, he went to hear one after another of the professors. He writes to his father:—"Mr Monro is by far the most graceful speaker among them, only the difficulty he sometimes has to recover himself after mistaking a word, makes it look as if his style were too much studied."

In May 1752 he returned to Aberdeen, and in October of the same year he went to London for the further prosecution of his studies. There he studied anatomy with Hunter, of whom he writes:—"I cannot help preferring his lectures to Monro's. He has much greater variety of preparations, takes more pains to have everything understood, and describes as if he wanted to inform himself more than us. He speaks with ease and fluency, and has a very genteel address. He allows everybody to handle and examine the preparations for himself. Mr Monro never allows them out of his hand, but he rather makes a greater number of practical observations than Hunter."—(*Letter to his father*).

He also attended two courses of Dr Smellie for the theory of midwifery. "The doctor is in appearance a dull, heavy-looking man; his lectures are not given in great good order, but distinct enough in the main. The principal benefit, indeed, is to be had from his machines, which are extremely ingenious, and imitate so exactly the natural birth, that he declares he owes most of what he knows with regard to turning children in preter-natural cases to practising upon them."—(*Letter, 20th November* 1852).

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* Read before the Royal Society.

He also attended occasionally St Thomas's Hospital and Dr Munckley's.

His stay in London, however, was very short, for before the end of January he set out for Paris. He expressed much regret that his father could not or would not allow him a longer period to prosecute his studies in London, and in this the friends to whom he had been introduced warmly sympathized. He regarded the French expedition as a very serious affair. He writes in one letter:—"I shall be in Paris like one dropt from the clouds;" and in another (Boulogne, 28th January 1753), mentioning various civilities he had received, he says, "My good fortune has been so extraordinary, that if I was a good enough Catholic I should call it a miracle. I parted from London with the heaviest heart I ever had, dreading every misfortune, yet in every step I have found a friend." Travelling was a serious undertaking in France in those days, for Skene mentions having journeyed by coach from Abbeville to Paris in four tedious days, a distance of about 100 miles.

After being a few days in Paris, he writes (February 15, 1753):—"Want of language is a prodigious loss to me, and obliges me to submit to a thousand inconveniences. I believe at least I shall learn patience here, though this virtue is not at all the common growth of the country. What I like worst of all is, that whenever I am to visit anybody, or go to any public place, I must sit two hours at least to be curled and powdered, and walk the streets with my hat under my arm. Nobody wears hats upon their heads, and the handsomest one you can buy costs sixpence, though a piece of old cloth breeches is rather politer."

There was no little difference between the manners and customs of Aberdeen and those of Paris in the luxurious days of Louis XV.

His father wrote to him to beware of religion and politics. He replies, "I have had many little disputes, but all in joke. I sit and hear myself given to the devil with great good-humour. The people here speak very freely on religious subjects, and always begin them themselves, and I contradict them so little that most of them have great hopes of my conversion."

From all this it must however be inferred that he had at least a tolerable acquaintance with the language before his arrival in the country, without which he could not have "disputed" a few days after, nor profited by the lectures he attended, unless such as were delivered in Latin.

Among his introductions he had letters to Abbé Gòrdon and Abbé Hook; the latter was very attentive to him. While in Paris, he attended the hospitals of L'Hotel Dieu and La Charité, and Monsieur Petit's course of operations, and practised with a *sage femme*. He also mentions (15th February) that the lectures of Monsieur Terreir and dissecting occupied much of his time.

The barbers of Paris at that time formed a considerable proportion of the surgical students. "The crowd and insolence of the perruquiers," he writes, "renders the attendance upon the hospitals always disagreeable, and very often useless" (12th March). And on the whole he appears to have been disappointed by the state of medical teaching in Paris, though it must be confessed he was not so long there as to be well qualified to judge. "Yet in all I have yet seen, there is nothing that is anyhow worth the expense and trouble but the dissecting alone. As soon as my money wears near an end I shall think of leaving this place, which I believe I shall be able to do without the least regret, as I am not at all fond of it." At the same time he was well aware of the shortness of the time he had been allowed to devote to his studies. "I am afraid it is necessary I should be out of sight for some time. To return to settle in Aberdeen after only six months' absence would seem not sufficient. If you approve it, therefore, I would come by Edinburgh, where I might stay a month or two at little or no expense."—(Paris, 12th March 1753). On 1st April he writes, "I have as yet most of the curiosities of Paris, with the king's country-houses, to see; and it will be necessary, I imagine, that I should be able to say I have seen these things."

From the same letter we obtain some information as to the estimation in which the Rheims degrees in medicine were then held. "With regard to taking a degree, I have made a good deal of inquiry about it here, and am informed, both by French and foreigners, that the least price at Rheims is ten guineas. The journey, which is thirty-six leagues, must cost four or five guineas, and fifteen guineas is certainly too much for a thing that is despicable even to a proverb; so much so here, that a man conceals his being a Rheims doctor like a crime. This was the case with Dr Farquharson. The high price surprised me, as I had often heard it mentioned at three guineas. I have indeed been told there are ways and means of procuring it under ten, but nobody can give me the smallest hint how it is to be set about; only, in general, that few succeed in that way but the Irish, who are rather more noted for poverty and assurance than with us, and commonly pass under the name of *Les Gascoigns d'Angleterre.* But if I can have a degree at Aberdeen for ten guineas, or perhaps for nothing, I would not choose to pay fifteen here."

He left Paris in the beginning of May; travelled by *chevaux de relais* to Dunkirk, in hopes of getting a passage direct to Scotland; but being disappointed in this, he proceeded to London, where he remained nearly a month, residing with Mr and Mrs Strachan, who showed him much kindness, and who seem to have had charge of him from his father. Mr Strachan is probably the well-known king's printer of that name, to whom many young Scotchmen were deeply indebted.

In writing from London (19th May 1753), he mentions with regret that he had not been able to attend lectures on chemistry and botany in Paris, "because there were no public lectures for the *first,* nor had those at the royal gardens for the last begun." "These," he adds, "are the only two branches of the business that as yet he had no lectures upon, and he expresses an anxious wish to remain a short time in Edinburgh on his way home, to have the benefit of Dr Alston's instructions.

His journey from London to Edinburgh was by land, in company with Dr Blackwell, Principal of Marischal College, and author of the "Court of Augustus." The journey was accomplished on horseback, and occupied nineteen days. He purchased a mare for L.8, 8s. in London, and sold her for the same price in Edinburgh. This journey gave him much pleasure. Just before starting, he writes, "The joy of having shunned the sea, and going through the finest country of the world with perhaps one of the most agreeable companions in it, make me expect this will be the most agreeable part of all my travels;" and he was not disappointed. At Cambridge they spent three days, and were present at the ceremony of conferring the degree of LL.D. on the Lord High Chancellor by the Duke of Newcastle, Chancellor of the University; and they made several visits and excursions from the direct road on their way. He was delighted by the appearance of the country. "Scotland was a truly mortifying sight after it." The whole journey, he states, cost him L.4, 4s.

In Edinburgh he remained two or three weeks, living with Mr James Burnett, afterwards Lord Monboddo, with whom he was very intimate, and attending Dr Alston's lectures in the Botanic Garden; but he was much annoyed by a letter from his father, complaining of the expense he had incurred, and requiring his immediate return. His reply is at once manly and dutiful.

Hitherto we have been able to trace his career from letters which have been preserved, but after this period it becomes much more difficult to mark his progress, because, unfortunately, few of his papers are dated.

He settled in Aberdeen as assistant to his father in July 1753, when not quite twenty-two years of age, and on 8th September of the same year he received the degree of M.D. from King's College and University. His father's practice included many of the principal families in Aberdeen and to a considerable distance around, and his necessary journeys through the country to visit patients afforded young Skene many opportunities of cultivating his favourite studies. His father lived till 1767, and of course up to that period Dr David had more leisure than he could otherwise have commanded. He never allowed his other studies to withdraw his attention from his professional avocations. A large volume of medical cases contains minute accounts of many diseases, and among others of his own gout. Fever, measles, and small-pox, appear from these notes to have peculiarly attracted his attention.

There was a plan of procuring his appointment as assistant to a medical professor in Aberdeen about 1764–5. The arrangement, however, failed, from his not approving the terms proposed; and it is only noticed as a proof that his favourite studies did not prevent his being regarded in the profession as one qualified by his talents and by his knowledge to teach others. For ten or twelve years after his settling in Aberdeen he pursued his many various extra-professional studies alone and unaided, except by such books as then existed; at least, among the letters preserved, we find no trace of correspondence on scientific subjects betwixt 1753 and 1765. During that time, however, much of his knowledge must have been accumulated, and many of his notes and descriptions of objects written.

The study of every branch of natural history in the north of Scotland a hundred years ago must have been a pursuit of knowledge under many difficulties, and Skene owed almost all his acquirements to personal observation,—a very little of it to the labour of others. A catalogue of his library has been preserved, and it was great and valuable for the time and place, extending to 600 or 700 volumes; not confined to works on medicine and natural history, but embracing a choice collection of the best Greek and Latin classics, and many of the standard works of English and French literature, proving that Skene was an accomplished general scholar. He was not a man to buy books he did not or could not use.

Skene was a very active member of the Aberdeen Literary and Philosophical Society. Several of the professors, both of King's and Marischal Colleges, were also members, and it appears to have been carried on with great spirit. A volume of papers and notes has been preserved, but unfortunately without dates. They are on many subjects, mostly metaphysical, moral, literary, or economical. Some of them are complete papers, others are only notes for speeches or abstracts of debates.

Dr Thomas Reid was one of the most energetic members of this Society until his removal to Glasgow, and the closest intimacy subsisted betwixt him and the two Skenes. It is very obvious that Reid imparted much of a metaphysical tone to its proceedings.

After Reid's removal to Glasgow he kept up a frequent correspondence with the Skenes; but as these letters were furnished to the late Sir William Hamilton, and most of them embodied by him in his life of Reid, they are here only noticed as illustrating the estimation in which Skene was held by the distinguished metaphysician. In 1766, Skene was admitted a member of the Philosophical Society of Edinburgh, but no trace appears among his papers of any communications by him to the Society. In 1767, Dr Hope made a proposal to resign the chair of Materia Medica in his favour on certain conditions. These were not approved after full consideration; but the correspondence shows that Skene was as much esteemed by the Edinburgh medical professors (mostly his own old teachers) as by his brethren in Aberdeen; and it is further very curious, as showing that neither party seems ever to have thought there was the slightest impropriety in selling and buying a professorship, and keeping the transaction strictly secret from the patrons. Though this arrangement failed, a constant correspondence was kept up betwixt Hope and Skene, mostly on botanical subjects. The introduction and cultivation of Turkey rhubarb was one frequent subject, for both regarded it as a matter of national importance.

Dr Reid endeavoured to persuade Skene to remove to Glasgow; but he showed no great desire to leave his native place, where his position, on the whole, was well suited to his tastes and pursuits. Skene only survived his father about three years, for he died in December 1770, at the age of thirty-nine, as appears from the inscription on his tombstone, near the south door of the West Church in Aberdeen. At the time of his death he was Dean of Faculty of Marischal College, and a letter has been preserved, from the Principal and Professors, wishing his funeral to be public. Whether it was so or not, cannot now be ascertained. He died unmarried; but tradition informs us that he was a very popular man, especially with the ladies; and that, with all his devotion to study, he did not neglect the means of making himself and his acquirements agreeable to others. From two undated papers, both apparently written about 1753 and 1754, he seems to have regarded the acquisition of knowledge and a good reputation among his fellow-men as the great objects of life. From occasional slight indications, it may be inferred that his religious opinions were deeply tinged with the lifeless rationalistic sentiments so prevalent in Scotland at the time.

Such is the meagre outline of Skene's life which we have been able to recover apart from his labours in the various branches of natural history.

It does not appear that up to the time of his settling in Aberdeen he had devoted any of his time to natural history, or had heard lectures on any department of it, except the two or three weeks during which he attended Dr Alston's lectures in Edinburgh. In every other branch of his favourite pursuit he must have explored the way for himself.

A considerable number of letters to Dr Skene have been preserved, and also abstracts of his own letters to his various correspondents, but, unfortunately, the latter are in general so much abbreviated, and written with so many contractions, as not to be easily deciphered.

His earliest distant correspondent on any branch of natural history was Mr Ellis, the well known author of the first really accurate work on corallines. The first letter is dated 1765, and a regular correspondence, with exchange of specimens, went on from that time till Dr Skene's death.

The nature of zoophytes engaged much of Skene's attention. "5th July 1765," he writes, "With regard to the nature and progress of the Sertularia, my sentiments entirely coincide with yours, as I think them supported both by analogy and facts. I have looked into Mr Baxter, but as yet can perceive nothing but a man with a plain road before him, bewildering and losing himself in a mist of his own making. Can anything be more pleasant than to find honest Job, overcome with the difficulty of discovering how the animal makes the cell, endeavouring to solve it by telling us that the cell makes the animal!

"There is a remarkable resemblance in many particular characters between the human epidermis and the outermost cortical tegument of vegetables. We have never yet been able to show how the epidermis is formed by the man, therefore I should not wonder to find some Dutch philosopher making the discovery that the man is formed by it.

"I am far from thinking that the operations of Nature should be limited by the very little wo know of them. She may have produced many modes of existence of which we are yet entirely ignorant; but for fear of being confined in our way of thinking, we should not run mad. When the proofs are nearly equal, the probability is always on the side of whatever is most analogous to the common course of nature. But in the present case, the proofs are far from equal; they seem to be all on one side. I wonder Linnæus has been perverted to the strange Dutch theory. I hope you will set him to rights. Indeed, upon examining his 'Sertularia,' there appear to be so many mistakes, that I suspect you will need to make out the whole article for him. Is it a fact, as Mr Baxter advanced, that different species of polyps are to be found in the same species of Sertularia? Have you ever observed any animals in the corallines of Linnæus? or do the proofs of the animal nature still depend on the structure and chemical analysis? Has anything more distinct occurred with regard to the sponges?"

Again, in November 1765, he mentions, "The polyps of the new coralline, whose vesicles you never saw till my specimen,—on it were some *Serpulæ spirorbis*, whose terodines I saw. This sight alone might satisfy the unprejudiced that both animals make their own cells. I have no doubt of the corallines of Linnæus being animals; yet I would wish to see the inhabitants . . . . . I think Linnæus wrong in the *Nereis pelagica*. It is not properly 'Acephala,' surely not 'Apoda,' as it uses the tentaculæ of Linnæus for feet, and crawls just like a scolopendra."

Many curious facts as to the then state of knowledge of the lower classes of animals might be gathered by one competent to the task from a minute examination of the correspondence betwixt Ellis and Skene.

When I originally promised to my much lamented friend, the late Dr Fleming, to attempt to write this memoir, I hoped to have had his invaluable assistance, especially in this branch of it—no one could have been found more competent—and he would have done it *con amore*, for he often expressed the deepest interest in Skene and in the manuscripts he left.

In December 1765, Skene wrote to the great Swedish naturalist, and received, what he terms in a letter to Ellis, "a very agreeable and complaisant letter from Linnæus;" and adds, "He does not seem quite satisfied about the nature of the Sertularia, &c., but is shy of giving an opinion. I am much pleased with his manner of writing."

Altogether, there have been preserved four letters from Skene to Linnæus, and three replies. One of Linnæus's letters appears never to have reached its destination.

As these letters throw considerable light on the state of various branches of natural science at the time, and also on the extent and accuracy of Skene's knowledge and his independence of mind, it is hoped that the Royal Society may be interested in seeing the originals. The seal used by Linnæus bears two twigs of his favourite *Linnæa borealis* hanging outside the shield, with the motto, "Famam extendere factis."

The twelfth edition of the *Systema Naturæ* was passing through the press at the time Skene and Linnæus were corresponding, and Skene is given as authority on several occasions; *e.g.*, *Aranea spinimobilis*, *Coluber jaculatrix*, *Anguis eryx*, *Serpula vermicularis*. We at the present day can hardly appreciate the value of such a work as the *Systema Naturæ* to a student like Skene.

Previous to the appearance of the twelfth edition, the whole of natural science was in confusion, and no one could well sit down to study any branch systematically without first making out a sort of system for himself.

Linnæus was gifted with a wonderful power of arranging and systematizing; and notwithstanding obvious defects and occasional blunders, his *Systema Naturæ* is a wonderful production, especially when we consider the want of accuracy and want of arrangement in the authors who preceded him. He had not the means of *generalizing* to the extent now enjoyed by the lovers of natural science, for the ascertained facts have increased an hundredfold since the days of Linnæus. Some writers of late have been inclined to scoff at Linnæus's difficulties as to the nature of zoophytes and of sponges; and yet, with all the accumulation of knowledge on the subject, he would be a very bold man who at the present day would venture to draw a clear, distinct line of *demarcation* betwixt animal and vegetable life.

The progress of the *Systema* was remarkable. It first appeared in 1735, in the form of twelve folio pages,—a mere outline, which was gradually filled up in successive editions, none, we believe, exceeding 200 to 250 pages, until the twelfth, which appeared in 1766–67, in three octavo volumes, comprehending an arrangement of the whole three kingdoms—animal, vegetable, and mineral. One may easily imagine the delight with which Skene would hail the publication of a work so fitted to assist him in all the branches of natural history, while, at the same time, his correspondence with Ellis and Pennant shows that he was ready to make manly and straightforward criticisms whenever he knew them to be just.

In 1769, Mr Pennant was introduced by Ellis to Skene, and a close correspondence was kept up during the remaining months of his life. Skene is repeatedly mentioned in his *Fauna Scotica*, prefixed to Lightfoot's *Flora*. Skene exhibited, in an eminent degree, one characteristic of a genuine naturalist, in his willingness to impart to others whatever knowledge he possessed, and whatever specimens he could procure.

By Dr Hope he was introduced to Lord Kaimes. They occasionally met when Kaimes came to Aberdeen on circuit, and occasional letters passed betwixt them. One is worthy of notice, as it contains very minute instruction to Lord Kaimes how to manufacture a moss dunghill.

In reply, 1st December 1766, Lord Kaimes writes: "I have already set on foot your receipt for a moss dunghill;" and the subject is more than once noticed in subsequent letters. Lord Kaimes and Lord Meadowbank have hitherto enjoyed the fame, such as it is, of being the inventors of moss dunghills, which belongs rather to Kaimes's ingenious correspondent; but whether it was a discovery of Skene's, or learned by him, cannot now be discovered. It is one instance of his intelligent practical attention to whatever came in his way. Two rather characteristic letters from Lord Buchan to Skene happen to have been preserved; one dated, "From my pleasing prison, the Pavilion, the day of my stupidity, 22d Septr. 1769."

It is impossible to know how much of Skene's correspondence has been preserved and how much has perished. Solander and others are alluded to as correspondents from whom no letters have been found. Every branch of natural history was more or less studied by Skene. There are extant many memoranda of meteorological observations, and of the temperature of his own body compared with that of the air; and he complains much of the inaccuracy of his thermometer. On two occasions, in November 1769, he records strange, and by no means laudable, experiments on himself. He sat down deliberately to intoxicate himself with rum punch, in order that he might note the effect of it on his pulse, which he did until his memoranda state that he was "very drunk." The result at which he arrives is, that the rum had little effect on the arterial system, but very great on the nervous.

The extent and importance of Skene's labours must be principally judged by the systematic manuscripts which have been preserved, and which, imperfect and defective as they are, present ample proof of his diligence and his accuracy. They are all in the form of notes and memoranda, written on backs of letters, or the blank spaces of returns of sick and wounded soldiers and sailors, an office of some emolument held by Skene, which seems to have been to act as medical inspector on the part of the Government.

Few of his notes are written out clean, but many occur over and over again, expanded and enlarged. In short, they exhibit to us the first rough outlines of everything, as made by one who was daily adding to its proper place every fact he could ascertain.

They leave little doubt that he contemplated a complete Fauna and Flora of his own neighbourhood, if not of the whole of Scotland.

Seven of these volumes are now produced. The largest is filled with botanical descriptions; another is filled with zoological descriptions of quadrupeds, birds, fishes, a few insects, and a considerable number of testacea, mollusca, and vermes; a third is nearly filled with entomological descriptions.

The whole are intended to be arranged after the system of Linnæus. The descriptions are very minute and elaborate; and had he lived to print, he would probably have greatly curtailed them. His object was to put down every particular, and thus give such a description as to leave no difficulty in identifying an object. But in doing this, a description too long and too minute is almost as likely to mislead as one too short and meagre. The great desideratum is, to lay hold of distinctive persistent marks, and avoid those which belong equally to various species, or which are variable in the species described; and it was in this that Linnæus generally excelled.

One set of papers. consisting of six discourses, "Of the Extent and Division of Natural History," deserves particular notice. They were read at different times before the Aberdeen Society, and very possibly were intended for the Edinburgh Society in their complete form. Several copies, more or less perfect, occur among Skene's papers, but that now produced appears to be his last edition, written with care, and containing his latest views and opinions. He alludes to it in his last letter to Ellis, only a few days before his death, and in fact that letter is mostly a condensation of the discourses.

They are now chiefly interesting as containing the views entertained, nearly a hundred years ago, by a shrewd, accurate, and independent observer of natural objects.

They may not be much valued by those actively engaged in the study of natural history as it is at the present day, but they can hardly fail to interest those who take pleasure in studying the gradual advance of human knowledge.

Letter from Dr David Skene *to* Linnæus, 16*th December* 1765.

Ex longo tempore ad te literas mittere decrevi fama tua præclara maxime permotus, præcipue vero ut gratias, quas possim, agerem viro celeberrimo, cui soli debeo, si quid incrementi, si quid voluptatis in scientia Naturali cepi. Per aliquot annos quando ex praxi medicinæ superfuit mihi aliquid otii investigationi Naturæ incubui, uti vero novis-

sima in hisce regionibus est ejus cultura defuit studiorum vel socius vel director; hinc irrepserunt mala retardatio, error frequens, dubia multa—nec habui quo confugerem nisi ad tua scripta et inventa; hisce solis quos feci progressus, debeo; denso vero agmine adhuc obscura proveniunt neque facili manu feliciter colenda est Natura. Incitatus vero ab amico meo eximo D$^{\text{mno.}}$ Ellisio (qui te maxime colit) ad te ipsum memet converto, sperans tibi non invisum fore, naturæ amatori favere et difficiliora elucidare. Digneris ergo orem ut gratias pro beneficiis jam acceptis nunc referram et quæ obscuriora occurrant in posterum proponam.

Aesa te præterita, indagandis zoophytis nostrorum littorum incolis occupatus fui. Quæ vero de hisce in regno tuo animali scripsisti, multa mihi dubia videntur. Naturæ instituto usitato nequaquam accomodare potui Animalia composita stirpe vegetante et zoophyta non autores suæ testæ, sed testam ipsorum. Demens certe (fatior) est Philosophia, quæ limites ponet, ultra quos Naturæ non licet excurrere; uti vero constante et regulari pede progredi vedetur natura, non temere admittenda sunt quæ huic progressui valde adversantur, nisi experientiis et observationibus minime dubiis comprobata. Nonne vero adest in zoophytis quasi duplex miraculum? Stirps vegetans producit flores animatos, et (uti omne animal et vegetabile ex ovo) a floribus illis animatis producenda sunt semina vegetabilia ex quibus alii nascantur stirpes vegetantes. Hac sane difficilia sunt quæ admittantur, nisi optime stabilita. Si vero zoophyta contemplamur ex legibus naturæ analogicis levior omnino videtur difficultas. Animalia sunt tenerrima, medullaria, injuriis etiam minimis facile destruenda, nisi tegmene vel corneo, vel calcareo, elastico, articulato, munita, cujus opere etiam in fluctibus marinis vitam incolumem transeant. Si obscurum est animalia tam simpliciter fabricata domos tam elegantes struere, multo sane obscurius videtur ut domi struant animalia. Paucis abhinc diebus in microscopio contemplatus sum sertulariam, quam nuper detexi et tibi forte adhuc non visam; casu affixæ fuere serpulæ aliquot spirorbes et magno cum oblectamento vidi et serpulæ teredines, et sertulariæ hydras, tentacula suo ad libitum exserentes, moventes atque retrahentes, tali quasi testimonio oculari perculsus, non potui non agnoscere quin utrumque animalejusdem fuerit indolis, æque et incola, et fabricator sui domicilii. En! Vir. Cl: quæ mihi de zoophytis evidentiora apparent, et ex ratiocinio et ex observationibus, ut in Epistola proponere liceat. Ad te vero provoco; obscura elucides, falsa corrigas vehementer velim. Felicissimum vero memet haberem, si quæ hoc arrident aliquo modo promovere possem. Plurimæ ex Ellisii Sertulariis in nostris littoribus inveniendæ sunt, aliquot etiam novas detexi, quas cum illo communicavi. Si specimena velis, fac sciam quomodo referenda sunt, et quam primum mittam; forte atque aliæ sunt Scotiæ productiones naturales, quæ tibi arriderent. Est nobis Anguis habitu ac colore prorsus idem ac Anguis Eryxa No. 262, satis exacte descriptus a Gronovio mus. 2$^{\text{do}}$, No. 9. Calculo non spermendo numeravi squamas abdominales 120, Caudales 137, longitudo ab apice rostri ad finem caudæ 15 unc., abmano ad finem caudæ circiter 8¼, caput speciminis paulo mutilati non bene observavi. E Surinama nuper accepi animalia plurima, et inter alios colubres 2 vel 3 specimina. C. scutis abdom. 163, quam caud. 77 Gronov. mus. 2$^{\text{do}}$, No. 26. Hic coluber tibi non visus memoratur in regno animali; si adhuc non possideas, mittam; accepi etiam Araneam (unicam vero) a te non memoratam, magnitudine A. aviculari vix cedit, crura spinis nigris nobilibus armantur, male pictum et pejus descriptum ab Albino No. 169 videtur. Oculi hoc modo positi sunt (⁚ ⋮ :). Si decsriptionem pleniorem velis, mittam. Ne vero taedeat Epistola longior, finem huic imponam. Sermone Latino, licet omnino ineleganti, intelligibili vero (uti spero) uti mallem quam Anglico, qui tibi minus forte in usu est.

Quando primum tuo commodo fiat rescribas vehementer optem. Valeas et me habeas observatissimum tui cultorem

Davidem Skene.

Inscribantur tuæ literæ
To Dr David Skene,
Physician in Aberdeen, N. B.

Letter from Linnæus *to* Dr David Skene, *21st January* 1766.

(Originals sealed with coat of arms bearing twigs of *Linnæa borealis*, as ornaments outside shield.)

Lætor magnopere quod tu, tamquam lucens Sidus, ortus sis in boreali Britannia, ubi nullum præter te curiosum novi.

Ad dubia a te, vir Clarissime, mota de natura zoophytorum non lubenter respondeo; lego cum oblectamento aliorum sententias, nec eas refello; dico tantum quæ mihi visa sunt; forte non semper tutissima; nec alios obstringo in meam sententiam.

Quænam est differentia inter vegetabilia et animalia? Anne sedes vitæ in medullari substantia? An plantæ sensû omni destituantur non dixi; nervos destitui, quibus motum voluntarium perficiunt, credo.

Fuci radicantur quasi in lapidibus, sed nutrimentum hauriunt non basi, sed per poros totius corporis. Isides et Gorgoniæ caulescunt ramis; hi rami, transversim dissecti, ostendunt corticem, ligneam (aut in quibusdam corneam substantiam ut in nonnullis) substantiam, annulis concentricis, annuis, et intra hanc substantiam medullarem.

Tænia inter utrunque articulum includit animalculum propria cute vestitum.

Sertulariæ videntur Tæniæ fixæ, in quibus infimi articuli, antea animati exaruere in substantiam fruticuli. Anne poteris cum tua sententia conciliare Elisii. Tab. V. *a*, *b*. Tab. VIII., Tab. IX. *n*. 17. Tab. XII. *n*. 19, 18. Tab. XIII. *a*. Tab. XVIII. *a*. Tab. XX. *a*, *b*. Tab. XXVII. *n*. 1.

Embryo humanus haurit succum ex placenta uterina ex utero matris et etiam ore, ut pullus in ovo gallino. Flosculos in zoophytis esse animatos, a motu spontaneo, et quod centro cibum ingerant, dubium esse nequit; quod hi flosculi in variis transeant in capsulas seminiferas, ut in plantis, docent plurima mea specimina ut in Ellisii, Tab. VII. b.

Sed his sepositis a te lubenter audirem tuam propriam de Spongia sententiam; in aqua dulci obtinui pulchram speciem globis cæruleis, quæ nulla ratione fabricant Spongiam.

Si me aliquibus Sertulariis rarioribus aliisve bene non graveris, ea servabo in tui memoriam; cum nave dirigantur Stockholmiam, deponenda in Telonio vectigali, ubi merces exonerantur apud earum rerum inspectorum Malmgren. Si vero mi literis tuis honorare velis, inscribantur Societati Regiæ Scientiarum Upsaliæ cujus societatis literas omnes ego aperio. Coluber tuus indigenus Angliæ 163–77, cum Gronoviano n. 26 perplaceret; potuit enim Gronovii eundem esse cum meo C. lineato, quem et ipse possideo et in musco regio attente vidi et examinavi, nec potest esse eundem.

Nec minus per placeret descriptio Aranei tui Spinis mobilibus, quem non vidi; si descriptionem velis mittere, oro quod brevi mittas, cum nunc sudet 12^{mo} editio Systematis in qua typhographus pervenit ad Anseres.—His vale et vive sospes tui Clarissimi nominis.

Cultor
Car^{v}· Linne, Equ. aur.

Upsaliæ 1766, ud. 21 Januarii.

Letters from Mr Ellis *to* Dr Skene.

Gray's Inn, *April* 25, 1765.

Sir,—I received your favour of the 17th of April, inclosing a specimen of your *Sertularia muricata*. It is entirely new to me, and shall certainly have a place in the second vol. I wish you could meet with a specimen more complete, that we might see the denticles; by a broken part of the stem, to which some of them adhere, they appear to be alternately placed. You inquire of me what Linnæus thinks or has wrote about these beings; for answer, I am persuaded he as yet knows nothing of the matter, for which reason I have told Dr Solander that we must sit down and arrange them properly for him, and distinguish between those parts called by Ray denticles, and what I understand by vesicles, which are properly the ovaries of these animals, the denticles being only the mouths by which they feed. The roots, as they appear, are only the first state of these animals while they are in that creeping form (which I have seen alive as the young animals fall from the vesicles); these fix themselves securely to some fucus, shell, or rock, and, from their radical state, they are empowered by nature to throw out several erect stems furnished with denticles, out of each of which a sucker or polype-like head appears to furnish nourishment for the future growth of the adult age; when this is advanced to a proper size, or perhaps age, the prolific state comes on, and then we find in regular rows (sometimes) the vesicles protruded; at other times I have met them very irregularly placed, as in the case of your *Sertularia*, which seems to have met with some injury by the violent agitation of the waves. I agree with you that Linnæus does not understand English, and his understanding or not understanding a letter from England depends on his interpreter.

I have lately had a very obliging letter from him, desiring me to give him all the information I can from the kingdom of Neptune, for that he is now publishing his "*Regnum Animale.*" I shall take what pains I can to set him right in things that regard myself, and am in hopes I shall alter his present system of zoophytes for the better. I have this evening received from Mr P. Collinson a very extraordinary sea production; 'tis in the shape of a crucible, hollow within. He told me it was a sea fungus from Norway; 'tis about thirteen inches high, ten inches and a half over the top by nine inches. The inward substance of it is like the crumb-of-bread-sponge, composed of an infinite number of masses of transparent minute spiculæ, with many irregular tubular passages through them; but all these minute tubular meanders terminate on the surface, which is composed of a cretaceous substance not unlike the *Corallina opuntioles* of Jamaica, and full of minute holes. The more I look into nature, the more I am puzzled; here is now an animal production between a sponge and the corallines.

I thank you heartily for the *Fucus piper*. I had it before in abundance. I collected it at the back of the Isle of Wight, and at Hastings, in Sussex, the last summer; but your *Baderlocks* I have never seen. It is particularly described in Caspar Bauhin's *Prodromus*, where it is called *Baderlocks*. His description exactly agrees with yours. I would not advise your sending it by sea; after a ship, it would not answer by any means. If you can get me a small specimen that is nearly the figure of the common large ones, let it be well washed in fresh water and then dried between linen cloths, and afterwards put into a book till it is dry enough to fold up in a letter not exceeding two ounces. When you have afterwards an opportunity to send me some dried specimens by ship from Aberdeen, I shall be glad of them.

Let your specimen of the *Baderlocks* be inclosed to P. C. Webb, and I shall receive it very safely.

Dr Solander has all this day been busy at the museum, showing the Duke of Athol and his family the museum, but is to come and spend a day in order to answer your very proper objections to Linnæus's *Zoophytes*. I shall then look for some specimens of *Sertularias* to send you, which you may expect in a post or two at farthest, as I have got a frank for that purpose. Pray try to get all the varieties you can, and send them inclosed to Mr Webb. I hope our correspondence will turn out to our mutual benefit.

I shall inquire in the city of the traders to your parts for a method of conveying larger bodies than the post can carry.—I am, Sir, your obliged humble servant,
JOHN ELLIS.

GRAY'S INN, *Dec.* 31, 1768.

DEAR SIR,—Your kind letter of the 17th inst. has given me great pleasure. I think you have treated Dr Pallas as he deserves, and exposed his quibbles in a masterly manner.

This, certainly, is the proper manner of reasoning with a philosopher who aims rather at perplexing than clearing up the point in dispute. You have beat him out of his subterfuge of QUASI, and reduced his reasoning to an absurdity. I have inclosed you a small piece of the horny part of a Gorgonia, divided lengthways, that you may see the course of the medulla. If you get a small young branch of a lime or elm tree, and cut it in the same manner, you'll soon be convinced of the difference of the medulla in one and the other.

If you make any further observations on his book, I hope you'll be so good as to communicate them to me.

I think you may make out a very good letter on the subject to be communicated to the Royal Society here, directed to me, as observations on Dr Pallas's "*Elenchus Zoophytorum*," which, I think, would do you honour; and if any figures are wanted, I shall take care to get them done. These hints of yours will likewise help me greatly in my introduction.

I don't know whether you have seen the last vol. of our Transactions; but it was those two letters of mine on this subject that procured me the medal.

I am sorry (as you observe) to see Linnæus still continue his distinction between *Lithophyta* and *Zoophyta*. I never made any, and have wrote to him often on the subject. But, as you observe, I should be sorry that anybody should treat him with severity as Pallas has done.

Pallas has a party in our Society; but believe me, they are greatly mortified at seeing his blunders exposed in my last papers, and will be more so if you send me a letter on the subject, containing the hints you have already sent, and what more you can collect in revising his work.

I shall send you all the characters of the genera of the zoophytes for your observations on them. I will do the best I can; but I am too sensible of my own inabilities in going through a work that requires good health and the vigour of youth, instead of the attempts of one that is past the grand climacteric.

If you want to be more particularly informed in any of the genera of zoophytes, I will explain the matter to you as well as I can. My best wishes attend you.—Dear Sir, your much obliged, obedient, and humble servant,
JOHN ELLIS.

Contributions to the Biography of Richard Trevithick, C.E. By R. Edmonds, Esq., jun., Penzance.

A distinguished man of old, to whom no statue had been raised, observed that he would rather men should ask, why a statue was *not* erected to him, than why it *was*. So, to the honour of Trevithick, the public are now inquiring why no account of his life and inventions has yet appeared, whilst persons who have done comparatively nothing for mankind have been rescued from oblivion by eminent biographers. One of the reasons, doubtless, is, that Trevithick was scarcely known except by his works, and few writers could produce a popular memoir out of such materials, unrelieved by those interesting personal details which constitute the very soul of biography.

By his discoveries in the generation and application of steam-power, he has perhaps done more for commerce and manufacture than any individual of the present century; for had he not lived, there might not have been to this day a railway in the world, nor a steam-boat plying on the open sea.

Ten years ago the Institution of Civil Engineers offered a prize for a memoir of Trevithick, which has not yet been claimed, although much has been published, in a fragmentary form, respecting him and his inventions, by various writers. To these fragments I can, from unpublished letters and other documents in my possession, make some interesting additions. I begin with a letter written six years since by the late Mr Michael Williams, one of the members of Parliament for West Cornwall, to a gentleman who was then collecting materials for a memoir of Trevithick. This letter is the more valuable, as it was evidently written for publication.

Trevince, near Truro, *5th January* 1853.

"Dear Sir,—I am favoured with your letter of the 31st ulto., enclosing one from Mr Francis Trevithick of the 24th idem., and have much pleasure in complying with your joint request to the best of my ability. I was well acquainted with the late Mr Richard Trevithick, having had frequent occasion to meet him on business, and to consult him professionally; and I am gratified in having the present opportunity of bearing testimony to his distinguished abilities, and to the high estimation in which the Cornish engineers of the day then regarded him. I need scarcely say, that time has not lessened the desire, in the county especially, to do him justice: as a man of inventive mechanical genius, few, if any, have surpassed him, and Cornwall may well be proud of so illustrious a son. At this distance of time, I can scarcely speak with sufficient exactness for your purpose of the numerous ingenious and valuable mechanical contrivances for which we are indebted to him; but in reference to his great improvements in the steam-engine I have a more particular recollection, and can confidently affirm that he was the first to introduce the high-pressure principle of working, thus establishing a way to the present high state of efficiency of the steam-engine, and forming a new era in the history of steam-power. To the use of high-pressure steam, in conjunction with the cylindrical boiler, also invented by Mr Trevithick, I have no hesitation in saying, that the greatly increased duty of our Cornish pumping-engines since the time of Watt is mainly owing; and when it is recollected that the working power now attained amounts to double or treble that of the old Boulton and Watt engine, it will be at once seen that it is impossible to overestimate the benefit conferred either directly or indirectly by the late Mr Trevithick on the mines of the county. The cylindrical boiler above referred to effected a saving of at least one-third in the quantity of coal previously required; and in the year 1812, I remember our house at Scorrier paying Mr Trevithick £300, as an acknowledgment of the benefits received by us in our mines from this source alone. Mr Trevithick's subsequent absence from the county, and perhaps a certain degree of laxity on his own part, in the legal establishment and prosecution of his claims, deprived him of much of the pecuniary advantage to which his labours and inventions justly entitled him; and I have often expressed my opinion that he was, at the same time, the greatest and the worst-used man in the county.

"As connected with one of the most interesting of my recollections of Mr Trevithick, I must mention that I was present, by invitation, at the first trial of his locomotive engine intended to run upon common roads, and of course equally applicable to tram and rail ways. This was, I think, about the year 1803; and the locomotive then exhibited was the very first worked by steam-power ever constructed.

"The great merit of establishing the practicability of so important an application of steam, and the superiority of the high-pressure engine for this purpose, will perhaps, more than any other circumstance, serve to do honour through all times to the name of Trevithick. The experiment, which was made on the public road close by Camborne, was perfectly successful; and although many improvements in the details of such description of engines have been since effected, the leading principles of construction and arrangement are continued, I believe, with little alteration, in the magnificent railroad engines of the present day. Of his stamping engine for breaking down the Black-rock in the Thames, his river-clearing or dredging machine, and his extensive draining operations in Holland, I can only speak in general terms that they were eminently successful, and displayed, it was considered, the highest constructive and engineering skill. As a man of enlarged views and great inventive power, abounding in practical ideas of the greatest utility, and communicating them freely to others, he could not fail of imparting a valuable impulse to the age in which he lived, and it would be scarcely doing him justice to limit his claims as a public benefactor to the inventions now clearly traceable to him, important and numerous as these are. From my own impressions, I may say, that no one could be in his presence without being struck with the originality and richness of his mind, and without deriving benefit from his suggestive conversation. His exploits and adventures in South America, in connection with the Earl of Dundonald, then Lord Cochrane, will form an interesting episode in his career; and, altogether, I am of opinion that the biography which you have undertaken will prove highly interesting and valuable, and I wish you every success in carrying it out.—Believe me, my dear Sir, yours very faithfully,

"MICH. WILLIAMS.

"E. Watkins, Esq.,
"London and North Western Railway,
"Euston Station, London."

The locomotive carriage referred to in this letter as "the very first worked by steam-power ever constructed," was also publicly and most successfully tried in presence of tens of thousands of spectators in the summer of 1803 in London, in the vicinity of the present Bethlehem Hospital, and the neighbourhood or site of Euston Square. These trials were on the common roads; but shortly afterwards, "in 1804, one of these locomotive engines was in use at a mine in Merthyr Tydvil, in South Wales, and drew on a tramroad as many carriages as contained about 10½ tons of iron, travelling at the rate of 5¼ miles an hour, for a distance of 9 miles, without any additional water being required during its journey."[*] This high-pressure engine of Trevithick, by which carriages are impelled on common roads and on railways, is also applicable to every purpose for which the low-pressure or condensing engines of Watt are exclusively applied, and it has been thus characterized by the eloquent Mickleham: "It exhibits in construction the most beautiful simplicity of parts, the most sagacious selection of appropriate forms, their most convenient and effective arrangement and connection, uniting strength with elegance, the necessary solidity with the greatest portability, possessing unlimited power with a wonderful pliancy to accommodate it to a varying resistance; it may, indeed, be called *The steam-engine.*" Mr Hebert, from whose work[†] I have taken this extract, adds: "*Such admirable combinations of inventive skill were never before contained in the specification of a patent;*" and Mr Clarke observes, that "In the establishment of the locomotive, in the development of the powers of the Cornish engine, and in increasing the capabilities of the marine engine, there can be no doubt that Trevithick's exertions have given a far wider range to the dominion of the steam-engine than even the great and masterly improvements of James Watt."[‡]

Trevithick's Early Life (1771–1816).—Richard Trevithick was born on the 13th of April 1771 in the parish of Illogan in Penwith, the most western Hundred of Cornwall. His father, being the purser of several mines, could have given him the best education that the neighbourhood afforded; but our young engineer had no taste for school exercises, and being the only son who survived childhood, was allowed by his parents to spend his time as he pleased, so that most of his boyhood was passed in strolling over the mines amidst which he lived, in observing their engines and machinery, and in conversing with the miners, engineers, and others, who could give him information about them. Yet, even in this manner, with scarce any schooling, and with no books, he acquired such practical knowledge of steam-engines and mine-machinery, that long before he attained his majority he was, to the utter astonishment of his father, appointed engineer to several mines. The father begged the mine-agents from whom the appointment had proceeded to reconsider what they had done, as he was sure his son could not at so early an age be qualified for so responsible an office. But having had sufficient proof to the contrary, they merely thanked him for his disinterested advice. In 1792 Trevithick was employed to test one of Hornblower's engines at Tincroft mine, near Redruth, and reported its duty as 16 to 10 over Watt's. Prior to this he had, with the assistance of William Bull (a workman previously employed in erecting Watt's engines in Cornwall), constructed several engines which did not come within the reach of Watt's patent.[§] Thus, at a very early age, Trevithick's great genius and self-acquired talents were practically acknowledged by the most competent authorities in Cornwall. Had he been throughout his boyhood a due attendant at school, he would doubtless have written a better hand and better English, and have qualified himself for succeeding his father in the lucrative office of a mine-purser. Fortunately, however, for mankind, his object was not to get rich, but to cultivate his inventive faculties (which he could not have done at school), and to let the world have the benefit of them, careless of his own personal interests. This, indeed, was throughout his life a prominent point of his character; and by neglecting to keep his discoveries within his own breast until patents for them had been obtained, others have had the credit for inventions suggested originally by himself.

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[*] Stuart on the Steam-Engine (1825), p. 164.

[†] A Practical Treatise on Railroads and Locomotive Engines. By Luke Hebert, C.E., Editor of the "Engineer's and Mechanic's Encyclopædia;" the "History of the Steam-engine;" of the "Register of Arts, and Journal of Patent Inventions," &c. (1837), p. 21.

[‡] The Railway Register for February 1847, edited by Hyde Clarke, Esq., pp. 87, 88. See also Stuart on the Steam-Engine (p. 162), who considers Trevithick's patent of 1802 "as forming an era in the history of the steam-engine."

[§] Railway Register for February 1847, p. 86.

On attaining his full stature, he stood more than six feet high, well formed, and without any tendency to corpulence. His muscular strength was such that he could lift two blocks of tin, placed one on the other, weighing seven cwts. He was unassuming, gentle, and pleasing in his manners; his conversation was interesting, instructive, and agreeable, and he possessed great facility in expressing himself clearly on all subjects. Occasionally a blunt expression would fall from him, particularly when obliged to go through an explanation a second time on account of the inattention or dulness of his hearer; on such occasions he would sometimes exclaim, or rather ask (for he had no idea of giving offence), "How are you so dull?" His dress was plain and neat, and his general appearance such that a stranger passing him in the street would have taken him to be some distinguished person.

His duties as engineer required him frequently to visit Mr Harvey's iron-foundry at Hayle, who invited him to his house and introduced him to his daughter, Miss Jane Harvey, only fourteen months younger than Trevithick. A mutual attachment was the result, and they were married on the 7th of November 1797. Her brother, the late Mr Henry Harvey, succeeded to the foundry, and became the most enterprising merchant in the west of Cornwall; to him the western part of the creek of Hayle is indebted for its extensive weirs and quays, and its vast reservoir, with tide-gates for clearing the mouth of the river from the sand which would otherwise choke it. All these works were constructed on a sandy plain, covered by the sea at every tide.

For nineteen years after their marriage, Mr and Mrs Trevithick lived very happily together in England; first at Plane-an-guary in Redruth, for a few months; then at Camborne, for ten years; afterwards in London, for two years; next at Penponds, in the parish of Camborne, for five or six years, at the house of his mother; and, finally, at Penzance, from which town he sailed for Peru on the 20th of October 1816, leaving behind him his wife, four sons, and two daughters, all of whom are still living. His two youngest sons adopted the profession of their father, and have acquired considerable distinction as civil engineers.

Whilst in London in 1816, preparing for his departure for South America, his portrait—a good likeness—was taken by Linnell. This half-length oil-painting (24 by 20 inches) has lately been presented to the South Kensington Museum, where it is suspended among the portraits of distinguished men—a painted copy and a photographic copy having been given in exchange for it. From this picture, and from a *post-mortem* plaster cast of Trevithick, Mr Neville Burnard, the Cornish sculptor, has made a marble bust, plaster copies of which adorn various institutions.

Most of his important discoveries were made before his departure for Peru. In 1802, while residing at Camborne, he, in conjunction with Mr Andrew Vivian, who supplied the pecuniary means, took out the patent for his celebrated steam-engine, and, in the same year, erected a small one "at Marazion, which was worked by steam of at least 30 pounds on the square-inch above atmospheric pressure. In 1804 he introduced his celebrated and valuable wrought-iron cylindrical boilers, now universally used in this county. . . . In 1811–1812 he erected a single-acting engine of 25-inches cylinder at Huel Prosper, in Gwithian, which, of course, had a cylindrical boiler, in which the steam was more than 40 lbs. on the square inch above atmospheric pressure; and the engine was so loaded that it worked full seven-eighths of the stroke expansively. . . . I believe (continues Mr Henwood, from whom I am quoting) I have now satisfactorily shown that Mr Woolf, instead of being the *first* to introduce the expansive action of steam in one cylinder, was *positively preceded* several years by Trevithick."* Trevithick was the first who turned the eduction-pipe into the chimney, as stated by Mr Gordon in his *Treatise on Elemental Locomotion*, by which means the draught in the chimney was greatly improved.†

Trevithick's attention had been engaged beneficially to the public on various other subjects besides the steam-engine before his departure for Peru; but as they have been noticed in other publications,‡ I will pass on to the introduction of his high-pressure engine into the mountains of South America.

Trevithick in South America (1816–1827).—Of his admirable steam-engine, patented in 1802, as already noticed, Trevithick had made a beautiful model—little dreaming, whilst making it, that it would be the means of introducing him into a new world for the exercise of his genius and engineering talents. Some very rich silver-mines in the mountains of Peru had been abandoned from the mere want of machinery to extract the water. Mr Uvillé, a Swiss gentleman, came to England from Lima in 1811 for the purpose of ascertaining whether any steam-engines could be successfully used in the rare atmosphere of those high mountains, and if so, whether they could be conveyed thither. Receiving no encouragement, he was about to return in despair, when, by mere accident, he saw this elegant model of Trevithick's high-pressure engine exposed for sale in a shop in London. Instantly the vast capabilities and simplicity, the enormous power and great portability of the machine, flashed upon his mind, and excited the most confident expectations of accomplishing his object. With this working model he hastened back to Lima, tried it in the highest elevations, found it perfectly successful, and having formed a company, took a second voyage to England to procure the necessary engines. A second time he was reduced almost to despair, for Boulton and Watt, the most distinguished engineers of their time, assured him that it was impossible to make engines of sufficient power small enough to be carried over the Andes; but Trevithick revived his hopes by undertaking to construct nine steam-engines of his own invention, in sufficiently small parts to be conveyed on the backs of mules from Lima to the mines of Pasco, a distance of about 150 miles. The "Wildman," South Sea whaler, in which these engines with various other materials were shipped, sailed from Portsmouth on the 1st of September 1814. From the invoice, still preserved, I find that four of these engines were for pumping, had cost very nearly L.1400 each, and were each of thirty-three horse-power; four others were winding engines, each of eight horse-power, the price of each being L.210; the ninth was a portable steam-engine of eight horse-power, used for rolling, and cost L.400. The freight of this cargo to Lima was L.1500, and the insurance L.2300. Trevithick contributed from his own purse a considerable portion of this outlay, for which, and for his services, a share of not less than one-fifth in the adventure was allotted to him. Mr Uvillé went to Lima with the engines, accompanied by three Cornish engineers, one of whom was William Bull, Trevithick's earliest partner. The engines were safely landed—transported across the mountains,—and, on the 27th of July 1816, the first steam-engine ever seen in South America was set to work at Santa Rosa, one of the mines at Pasco. The Lima Gazette of the 10th of August 1816, in announcing this fact, says: "We are ambitious of transmitting to posterity the details of an undertaking of such prodigious magnitude, from which we anticipate a torrent of silver that shall fill surrounding nations with astonishment."

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* Philosophical Magazine and Annals of Philosophy for August 1831, in a letter to Richard Taylor, Esq., F.S.A., &c., by W. Jory Henwood, Esq., F.G.S., &c., p. 97.

† Hebert on Railroads, &c., p. 25.

‡ The following is an extract from the Catalogue of the South Kensington Museum under the name of Trevithick: "Inventor and constructor of the first high-pressure steam-engine, and the first steam-carriage used in England; constructor of a tunnel beneath the Thames, which he completed to within a hundred feet of the proposed terminus, and was then compelled to abandon the undertaking; inventor and constructor of steam-engines and machinery for the mines of Peru (capable of being transported in mountainous districts), by which he succeeded in restoring the Peruvian mines to prosperity; also of coining machinery for the Peruvian Mint, and of furnaces for purifying silver-ore by fusion; also inventor of other improvements in steam-engines, impelling-carriages, hydraulic-engines, propelling and towing vessels, discharging and stowing ships' cargoes, floating-docks, construction of vessels, iron buoys, steam-boilers, cooking, obtaining fresh water, heating apartments, &c. Patents, Nos. 2599 (1802), 3148 (1808), 3172 (1808), 3231 (1809), 3922 (1815), 6082 (1831), 6083 (1831), 6308 (1832)."

On the 20th of October in the same year (1816), Trevithick sailed from Mounts-Bay in another South Sea whaler with more machinery, and landed at Lima on the 6th of February following, where he was immediately presented to the Viceroy of Peru, and received the most flattering attention from the inhabitants. The Lima Gazette of 12th February (now before me), after noticing the completion of a second engine, with a detail of the wonderful effects produced, thus proceeds: "To this agreeable intelligence we must add that of the arrival at Callao of the whale-ship "Asp" from London, having on board a quantity of machinery for the Royal Mint, and for constructing eight steam-engines equal to those already erected in Pasco. But the most important intelligence is the arrival of Don Ricardo Trevithick, an eminent professor of mechanics, machinery, and mineralogy; inventor and constructor of the engines of the last patent, who directed in England the execution of the machinery now at work in Pasco. This professor, with the assistance of the workmen who accompany him, can construct as many engines as shall be wanted in Peru, without sending to England for any part of these vast machines."* The following is an extract from a private letter of Trevithick on this occasion:—"The Lord Warden was sent from Pasco to offer me protection and to welcome me to the mines. They have a Court over the mines and miners the same as the Vice-Warden's Court in England, only much more respected and powerful. The Viceroy sent orders to the military at Pasco to attend to my call, and told me he would send whatever troops I wished with me. As soon as the news of our arrival had reached Pasco, the bells rang, and they were all alive, down to the lowest labouring miner, and several of the most noted men of property have arrived here (150 miles) on this occasion, and the Lord Warden has proposed erecting my statue in silver."

What treasures were yielded by the mines before the civil wars put a stop to them, I do not know; nor am I aware how Trevithick afterwards employed himself, although it appears that he joined Earl Dundonald (then Lord Cochrane), and was for some years with him in South America. At length, he returned to England, having crossed the Isthmus of Panama, encountering hairbreadth escapes, and extraordinary adventures, and landed in Falmouth in complete destitution on the 9th of October 1827.*

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* This is a literal translation of the passage.

Whilst with Lord Cochrane, he invented a most ingenious gun-carriage, of which he showed me a beautiful model. By this invention (described below) "a single-decked ship will carry a greater number of guns on one deck than a double-decked ship on both decks, be worked with less than one-third of the hands, and the guns fired with precision five times as fast as they are at present. A frigate would mount very conveniently fifty 42 lb. guns on one deck with 150 men, and would discharge, with much greater precision, more weight of ball, in the same time, than five 74 gun-ships."† What has become of this iron gun-carriage I cannot learn, nor whether it was ever tried in the navy.

Whilst crossing the isthmus of Panama, he made notes and maps of the best line in which a road or canal might be made to traverse it. These are still in the possession of his family.

Trevithick's claims on his country.—The first thing to which Trevithick applied himself on his return from South America was to replenish his purse. Justly considering himself entitled to remuneration from his country, for the great benefits derived from his inventions, he furnished my father (his solicitor) with instructions for a petition to the House of Commons for that purpose. The petition was prepared accordingly in December 1827, and the following are extracts from it:—

"*That* this kingdom is indebted to your Petitioner for some of the most important improvements in Steam-engines, for which he has not hitherto been remunerated, and for which he has no prospect of being ever remunerated, except through the assistance of your Honourable House.

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* Since writing the above, I have seen the Supplement to the Mining Journal for 12th February 1859, containing some account of Trevithick during his absence in South America, from which the following are extracts:—"The patriots kept him up in the mountains as a kind of patron and protector, and the royalists looking upon Trevithick as the great means whereby the patriots obtained the sinews of war, ruined his property wherever they could, and mutilated his engines. . . . It is said that he had to make his escape, and after great difficulties succeeded. He then visited various parts of the west coast; but it appears that the last four years were chiefly spent in Costa Rica, in the countries now so well known as the route of the Nicaraguan transit, and the scene of General Walker's filibuster warfare, where he engaged in mining with his friend Mr John Gerard."

†"This gun is worked by machinery, centred and equally balanced, like a crane on pivots, which give it universal motion, by which plan it is worked by one man only, with as much facility, precision, and ease, as a soldier's musket. One man is placed on one side of the gun to put a copper charger of powder into the muzzle, and another on the opposite side to drop a ball (in a bag) down the gun as it stands on its end, the man who sits on the seat behind the gun points it and pulls the trigger.

"The gun, on being fired, runs in and up an inclined plane, at an angle of 25 degrees, for the purpose of breaking the recoil, and runs down this inclined plane again with its muzzle out through the port; it requires no wadding, swabbing, cartridge, or ramming, but runs in, out, primes, cocks, shuts the pan, and breaks the recoil of itself, and can, by the help of only three men, be fired three times every minute with accuracy. The gun-carriage or case to be made a tube, of three feet long and three feet diameter, of wrought iron, a quarter of an inch thick, centred on a pivot to the deck, and the gunner seated on a bench affixed to the case and looking over the gun through the case. However great the swell may be, both gun and gunner are always steady to their place. When the gun is housed, it is hooked fast to the side of the ship, which effectually secures it. As this gun will not require any tackle, and only one man on each side of it, only a space of five feet six inches is required from the centre of one port to the centre of the next."

"*That* the duty performed by Messrs Boulton and Watt's improved steam-engines in 1798 (as appears by a statement made by Davies Gilbert Esq., and other gentlemen, associated for that purpose) averaged only 14½ millions pounds of water lifted one foot high by one bushel of coals, although a chosen engine of theirs, under the most favourable circumstances, at Herland, while mine lifted 27 millions, which was the greatest duty ever performed till your Petitioner's improvements were adopted, since which, the greatest duty ever performed has been 67 millions, being much more than double the former duty.

"*That* prior to the invention of your Petitioner's boiler, the most striking defect observable in every steam-engine was, the form of the boiler, which in shape resembled a tilted waggon, the fire being applied under it, and the whole being surrounded with mason-work. That such shaped boilers were incapable of supporting steam of a high temperature, and did not admit so much of the water to the action of the fire as your Petitioner's boiler does, and were also in other respects attended with many disadvantages.

"*That* your Petitioner, who had been for many years employed in making steam-engines on the principle of Boulton and Watt, and had made considerable improvements in their machinery, directed his attention principally to the invention of a boiler which should be free from these disadvantages, and after having devoted much of his time, and spent nearly all his property in the attainment of his object, at length succeeded in inventing and perfecting that which has since been generally adopted throughout the kingdom.

"*That* your Petitioner's invention consists principally in introducing the fire into the midst of the boiler, and in making the boiler of a cylindrical form, which is the form best adapted for sustaining the pressure of high steam.

"That the following very important advantages are derived from this your Petitioner's invention. This boiler does not require half the materials, nor does it occupy half the space required for any other boiler, no mason-work is necessary to encircle it, accidents by fire can never occur, as the fire is entirely surrounded by water, and greater duty can be performed by an engine with this boiler (and with less than half the fuel) than has been accomplished by any engine without it. These great advantages render this small and portable boiler not only superior to all others used in mining and manufacturing steam-engines; but likewise the only one which can be used with success in steam-vessels or steam-carriages. The boilers in use prior to your Petitioner's invention could never, with any degree of safety or convenience, be used for steam-navigation, as they required a protection of brick and mason-work to confine the fire with which they were surrounded, and still there was danger of accidents by fire resulting from the rolling and pitching of the ship in a storm.

"That, had it not been for your Petitioner's invention, the late important improvements in the use of steam could not have taken place, as none of the old boilers could have withstood a pressure of more than 6 pounds to the inch beyond the atmospheric pressure, whilst your Petitioner's boiler is not only very commonly worked at a pressure of 60 pounds to the inch, but is capable of withstanding a pressure of above 150 pounds to the inch.

"That as soon as your Petitioner had brought his invention into general use in Cornwall, and had proved to the public its immense utility, he was obliged in 1816 to leave England for South America, to superintend extensive silver mines in Peru, from whence he did not return until October last. That at the time of his departure, the old boilers were rapidly falling into disuse, and when he returned they had been generally replaced by his own.

"That the engines in Cornwall (which are more powerful than those used in any other part of the kingdom) have now your Petitioner's improved boilers, and it appears, from the monthly reports, that these engines, which in 1798 averaged only 14½ millions, now average three times that duty with the same quantity of coals, making a saving to Cornwall alone of about L.100,000 per annum; and an engine at the Consolidated Mines, in November 1827, performed 67 millions, which are 40 millions more than the duty performed by Boulton and Watt's chosen engine at Herland, as before mentioned.*

"That, but for your Petitioner's invention, the greater number of the Cornish mines, which produce nearly L.2,000,000 per annum, must have been abandoned.

"That your Petitioner has also invented the iron stowage water-tanks and iron buoys, now in general use in His Majesty's Navy and with merchant ships. That 25 years ago your petitioner likewise invented the steam-carriage.

"That all the inventions above alluded to have proved of immense national utility, and your Petitioner has not been reimbursed the money he has expended in perfecting them.

St Erth, Hayle, *December* 1827."

The letter from Trevithick to my father, enclosing the instructions for this petition, was dated the 20th of December 1827, and contained the following postscript:—"I was at Dolcoath account on Monday, and made known to them my intention of applying to Government, and not to individuals, for remuneration. They are ready to put their signatures to the petition, and so will all the county." This readiness of all Cornwall to support him in his application to Parliament shows how greatly he had benefited his native county, and how little he had been rewarded for it; that he was, indeed, as Mr Michael Williams so often said, "the greatest, and, at the same time, the worst used man in Cornwall."

Soon after the petition had been prepared, Trevithick met with a partner who supplied him with all the money he required for perfecting his never-ceasing inventions. This being all he wanted, the petition was never presented, and he gladly resumed the kind of life which he had pursued for so many years with so much success in Camborne, when in partnership with Mr Vivian. Thus assisted, he obtained a patent in 1831 for "an improved steam-engine;" and another, in the same year, for "a method or apparatus for heating apartments;" and a third on the 22d of September 1832, for "improvements on the steam-engine, and in the application of steam-power to navigation and locomotion." This was the last patent he took out, and "he died at Dartford in Kent, on the 22d of April 1833, leaving no other inheritance to his family but the grandeur of his name and the glory of his works."†

Hitherto, however, the public have been much less familiar with "the grandeur of his name" than with "the glory of his works;" for whilst he lived he was so little known, so exclusively occupied with his inventions, and so careless of his personal interests, that, independently of his literary disqualifications, he had neither time nor inclination to be the herald of his own achievements, and therefore some of his great inventions were (particularly during his eleven years absence in South America) strangely ascribed to others. But as he was clearly the inventor, not only of the high-pressure steam-engine and the steam-carriage, but also of that boiler without which (or a modification of which) no steam-boat could have ventured to cross the Atlantic, he has undoubtedly contributed more to the physical progress of mankind than any other individual of the present century.

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* In all the Cornish mine-engines the steam is *produced* by Trevithick's boiler and *reduced* by Watt's condenser.

† Railway Register for February 1847, p. 96.

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A Chapter on Fossil Lightning. By Dr GEORGE D. GIBB.—*From the Author.*

Cause of Rain and its Allied Phenomena. By G. A. ROWELL. Oxford, 1859.—*From the Author.*

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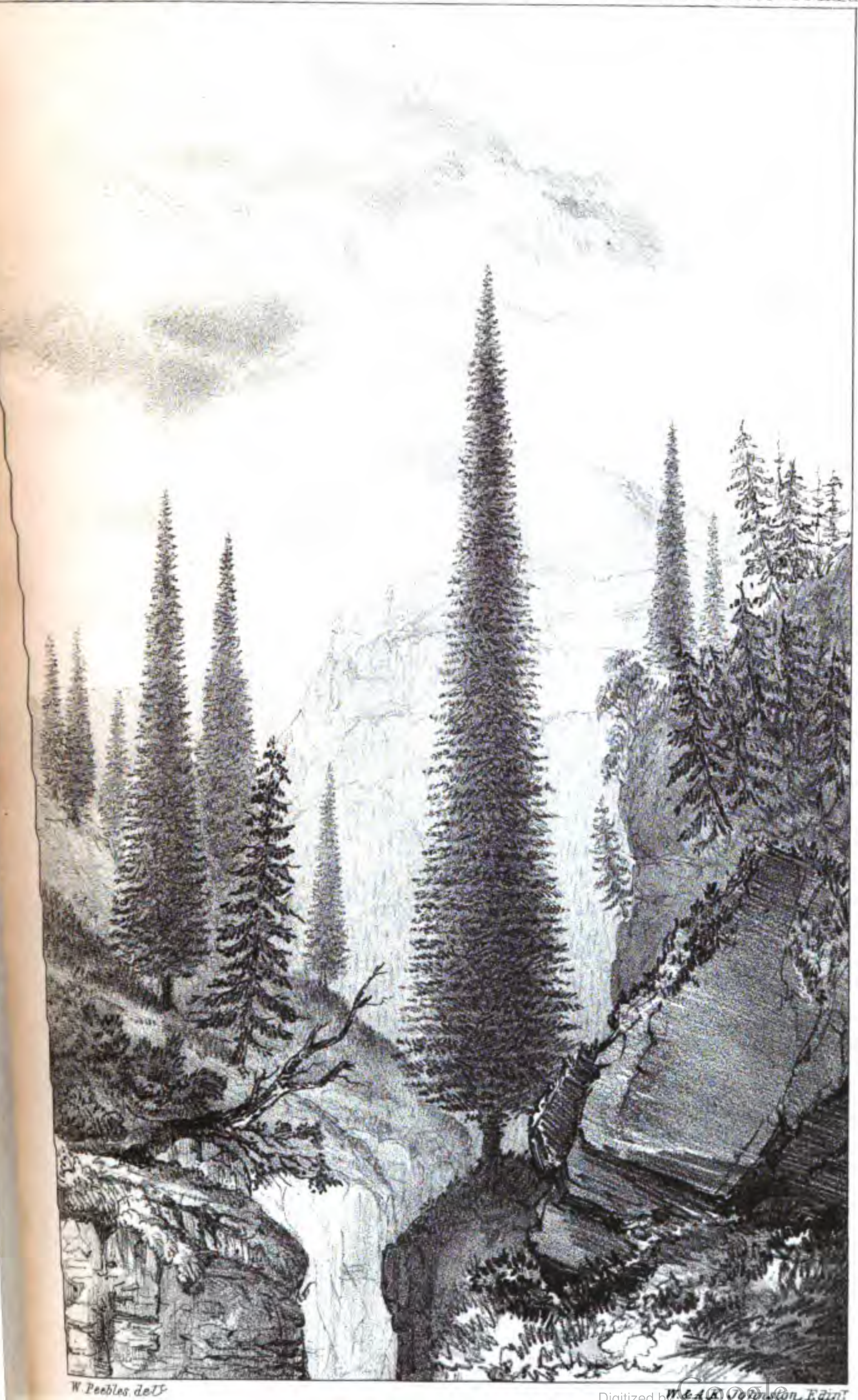

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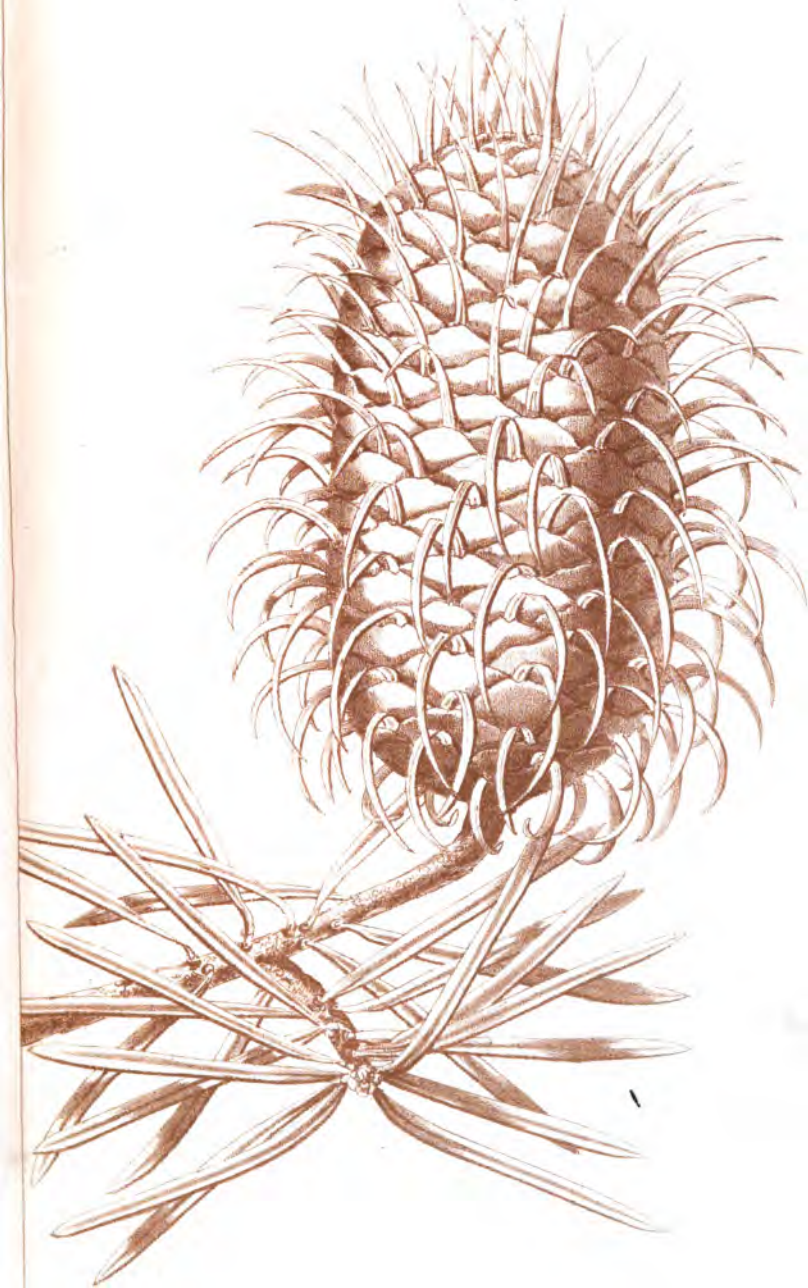

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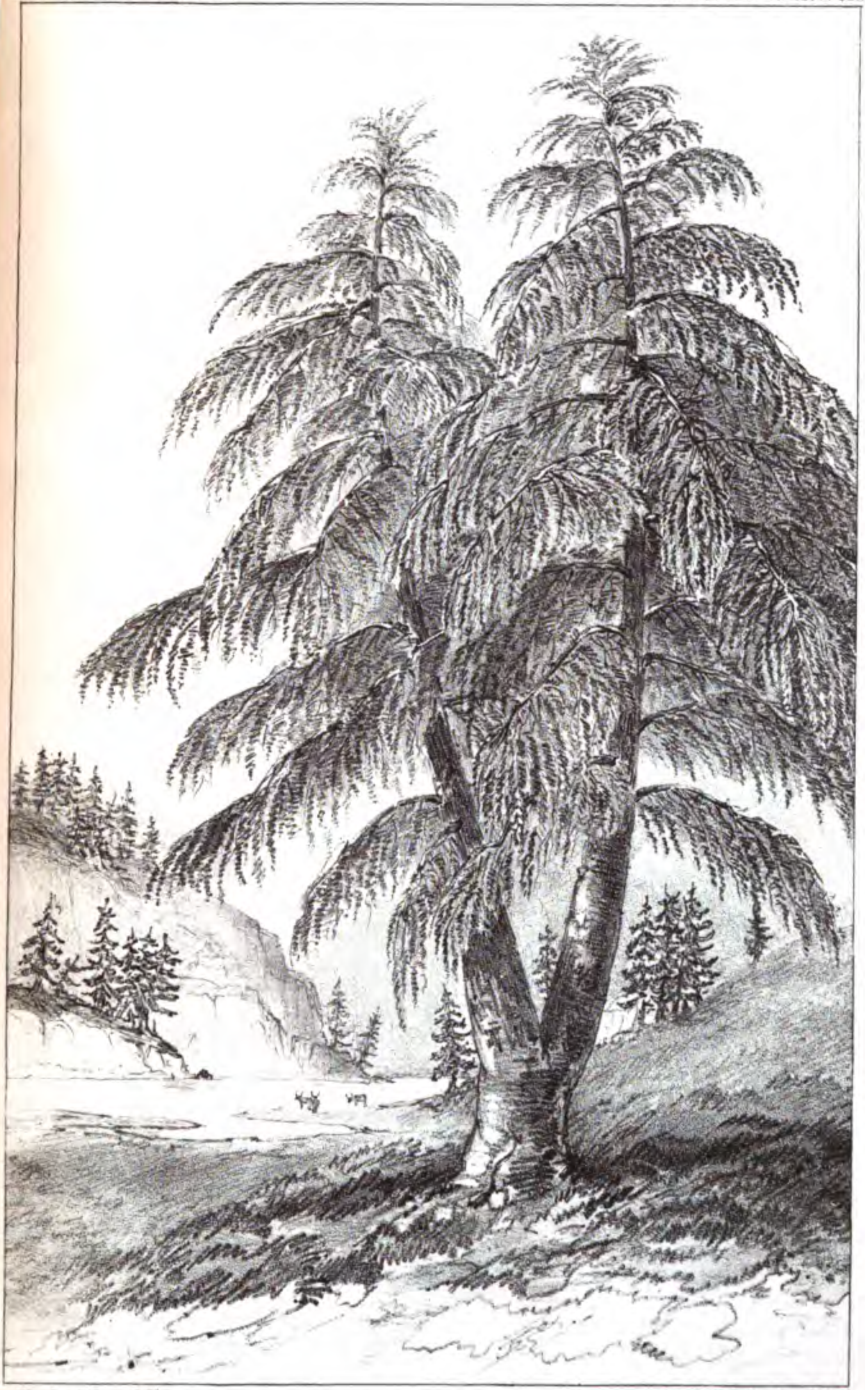

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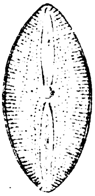
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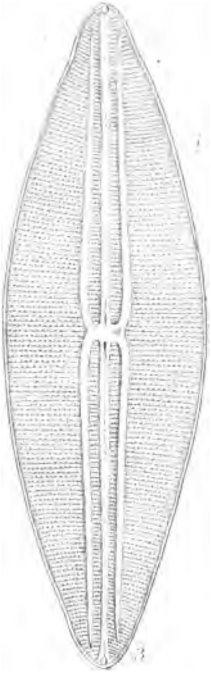
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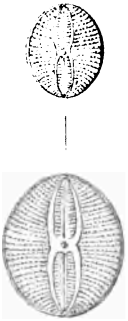
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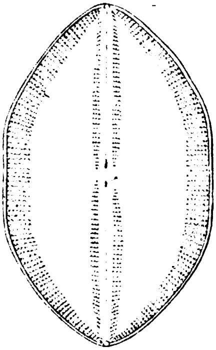
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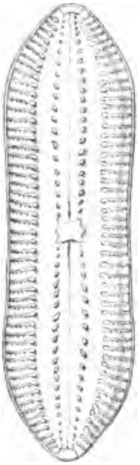
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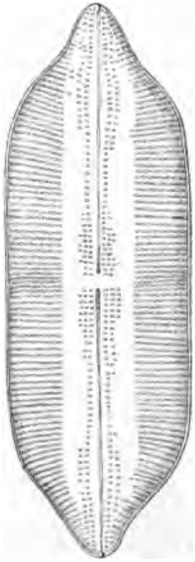
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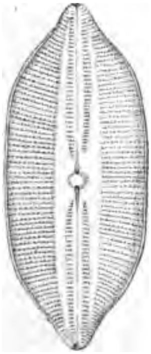
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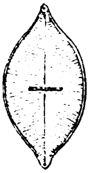
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W. & A. K. Johnston

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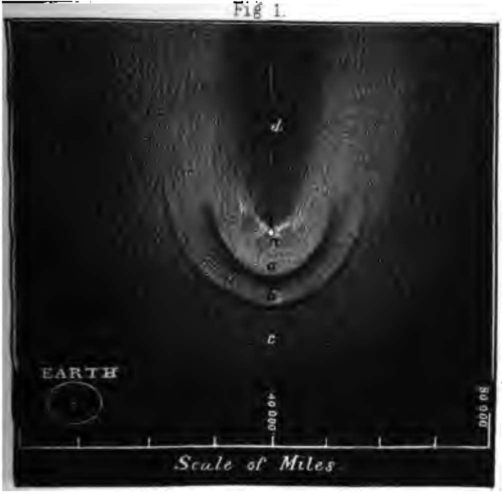

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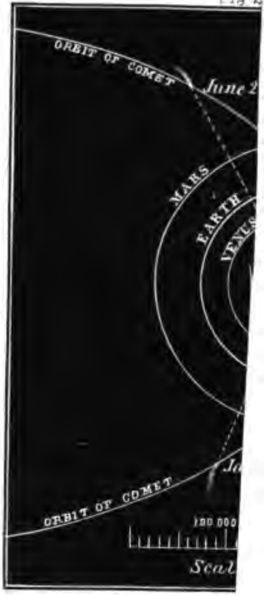

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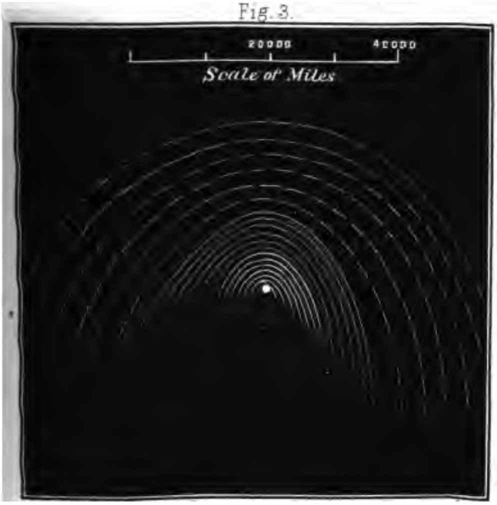

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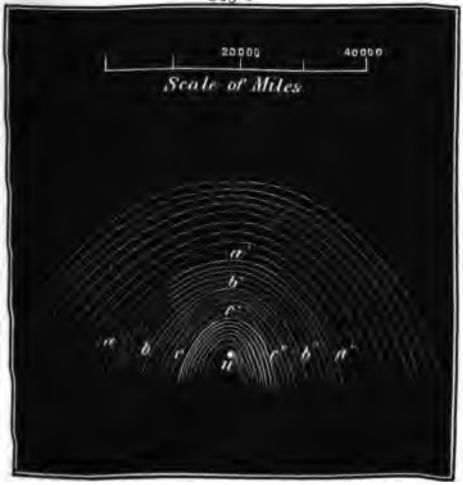

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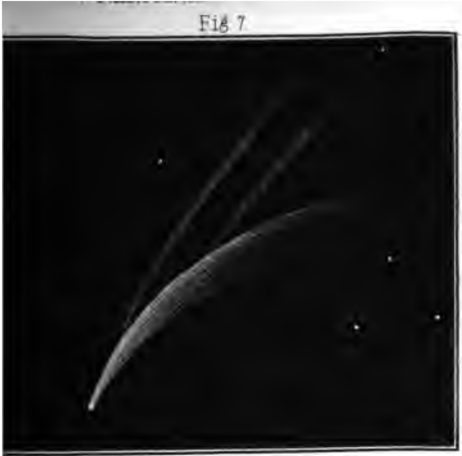

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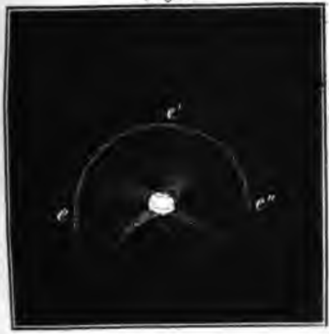

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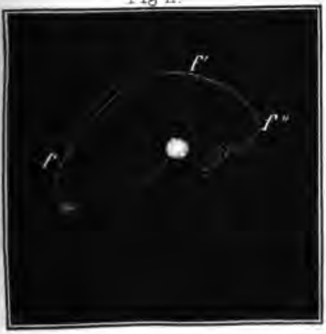

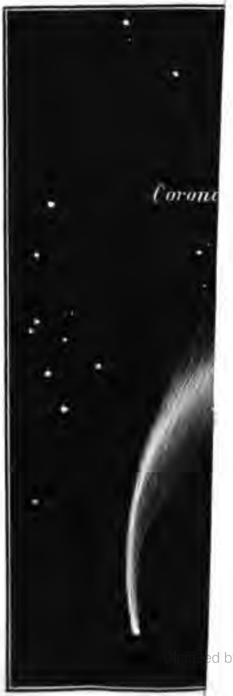

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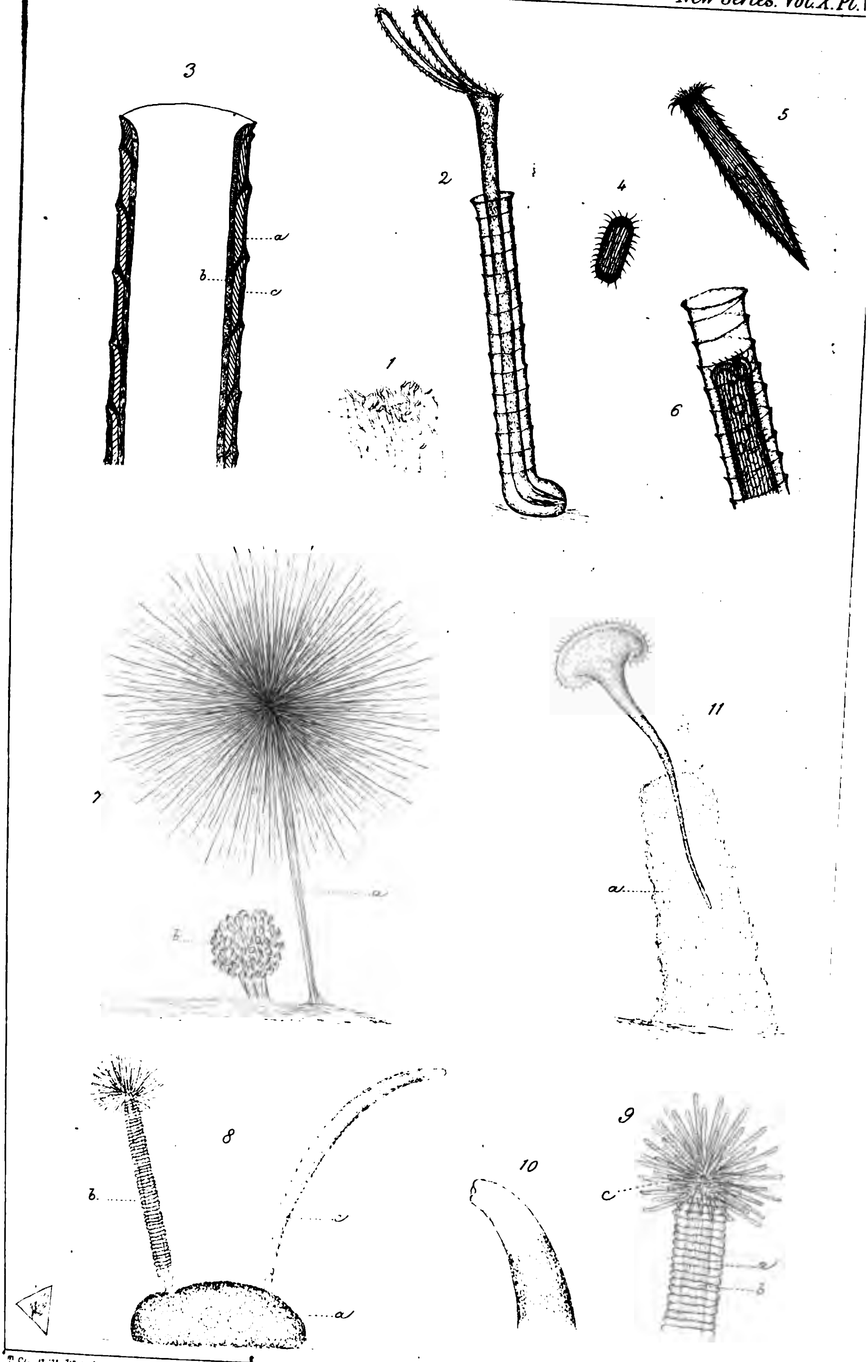

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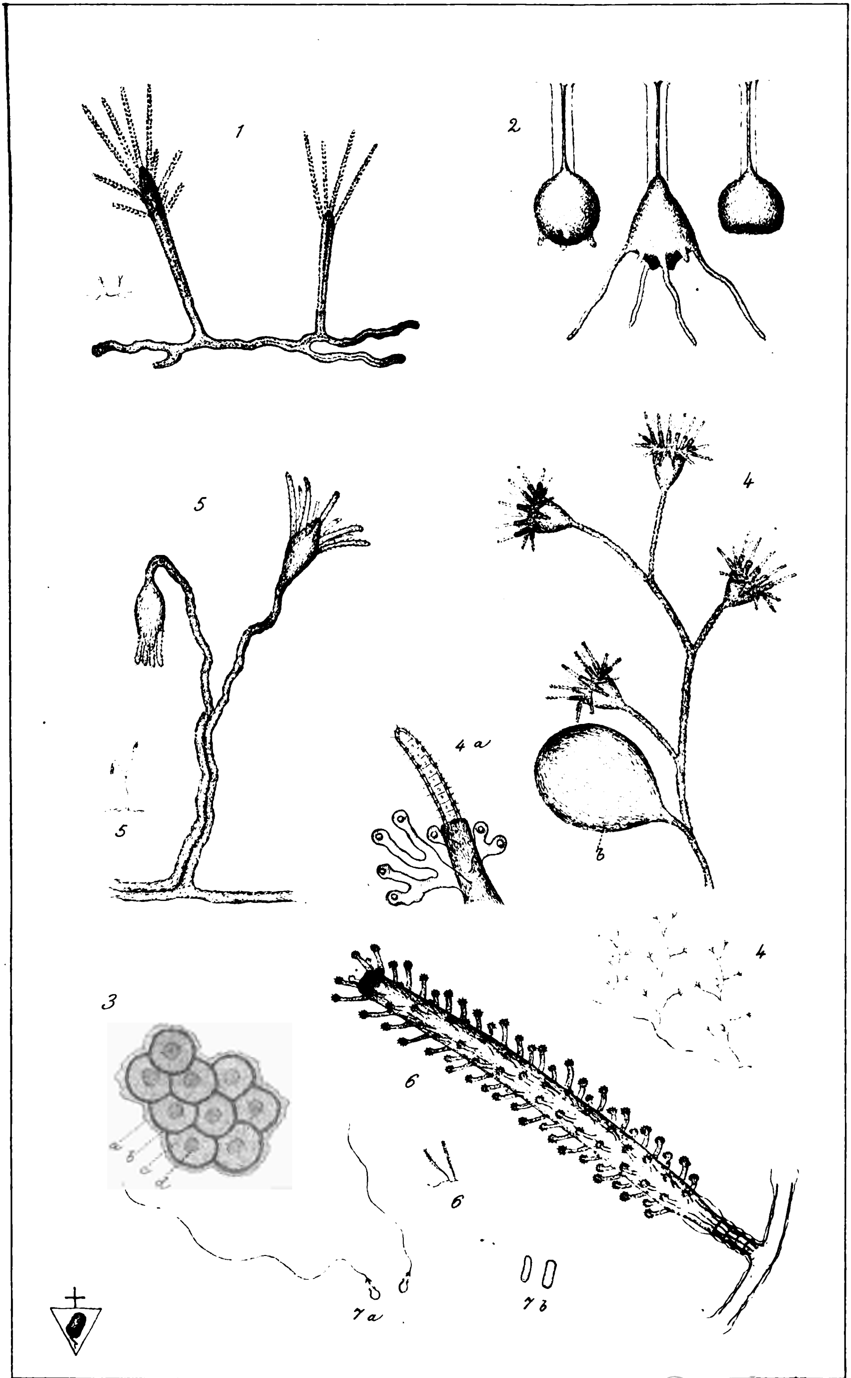

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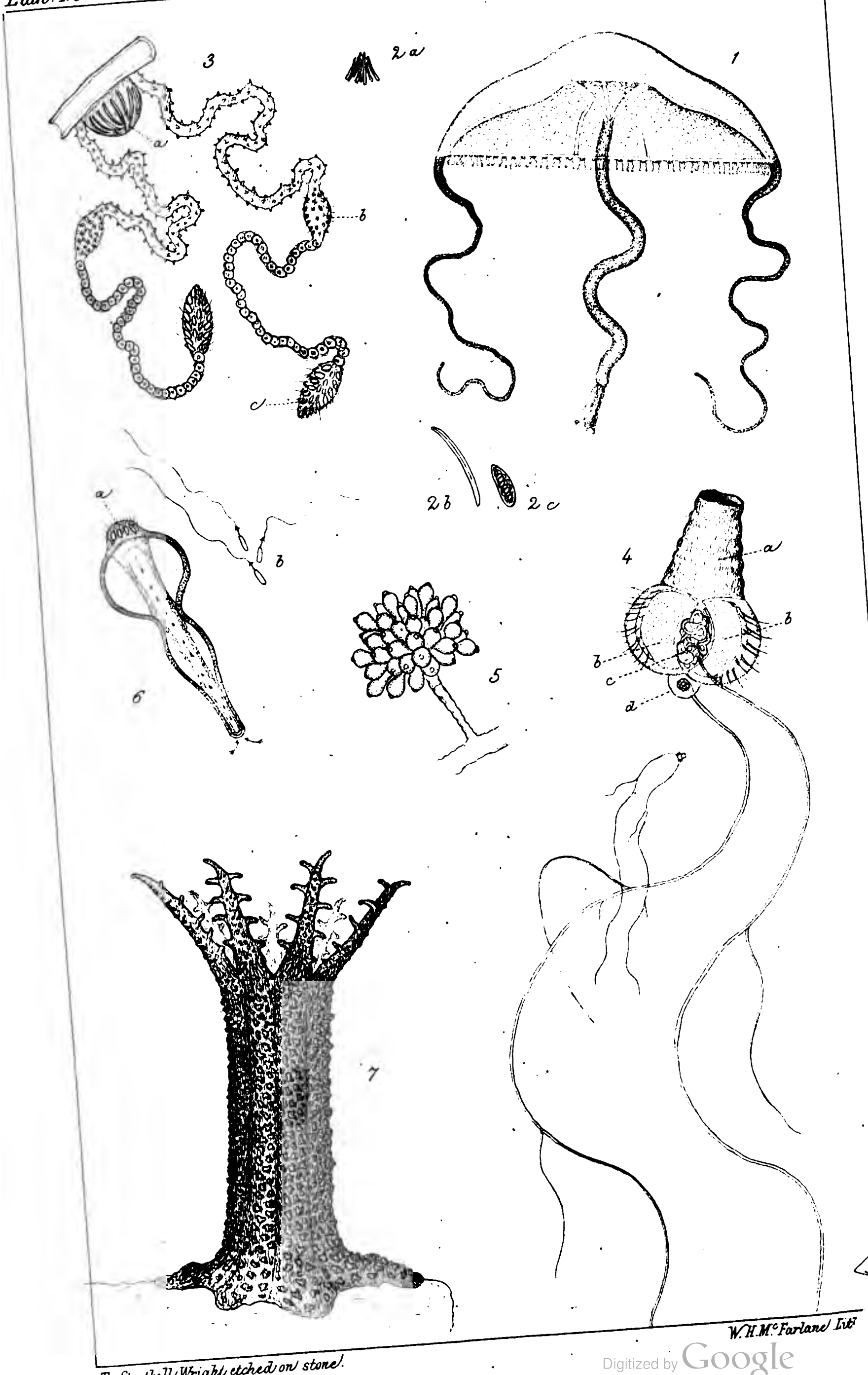

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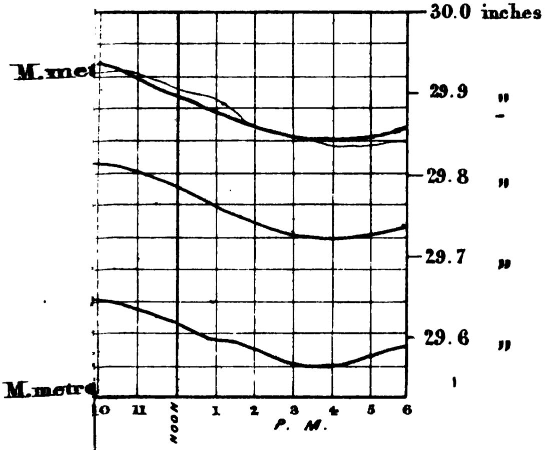

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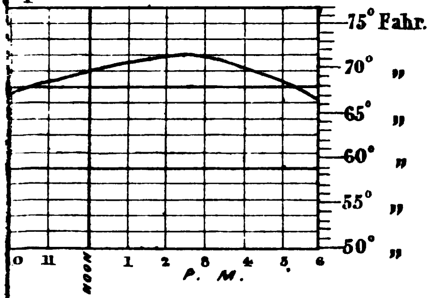

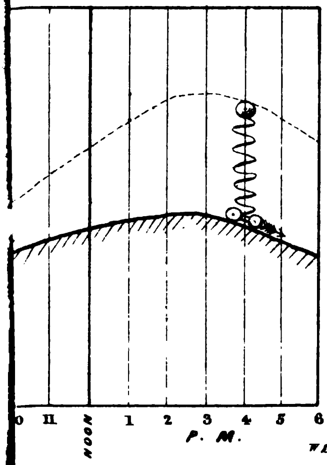

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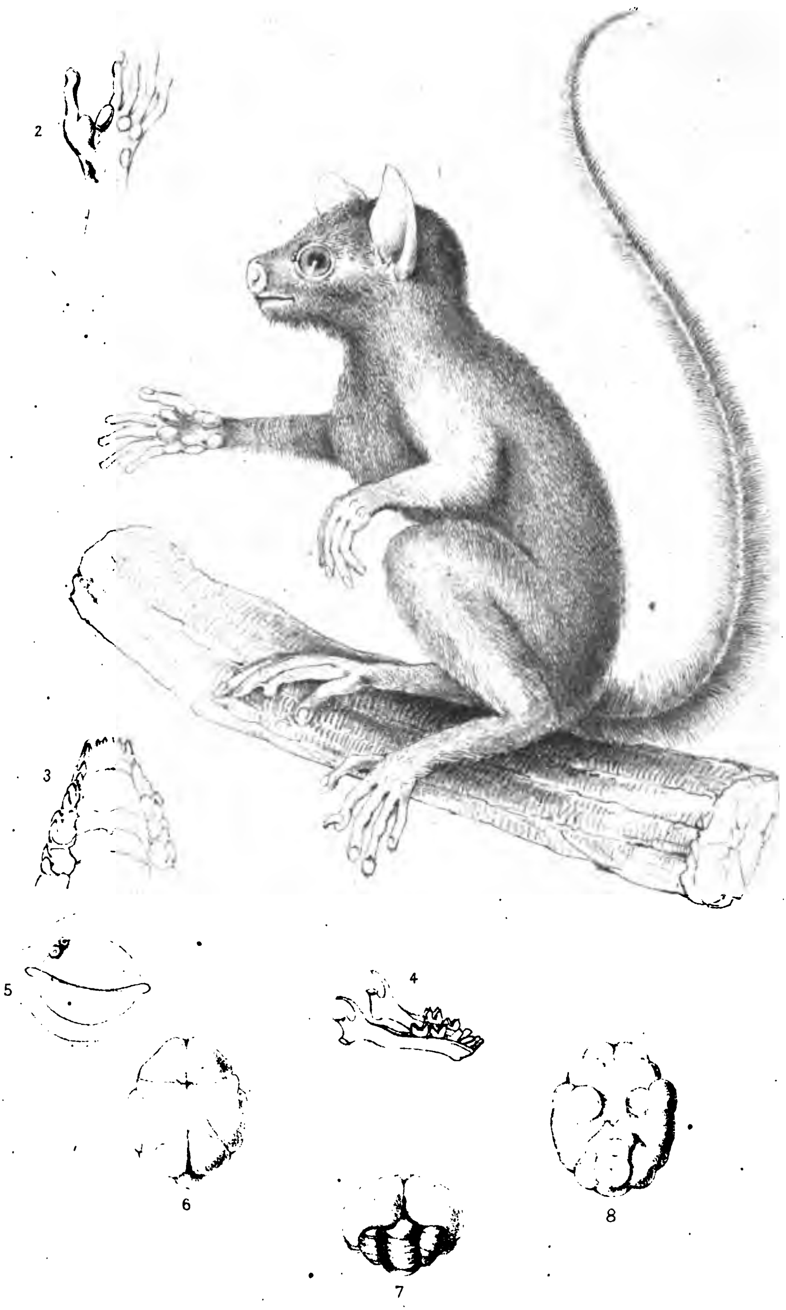

W. & A. K. Johnston Edi

Galago Murinus (Murray)

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